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CONTENTS

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THE PSYCHOLOGICAL REVIEW

THEORIES OF INHIBITION

PART II

THE REFRACTORY PHASE HYPOTHESIS OF INHIBITION

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Origin.—The hypothesis that inhibition is due to the development of a more or less protracted refractory phase in the relevant tissues originated with Verworn.[1] Forced to abandon the Gaskell-Hering hypothesis of assimilative inhibition because of lack of experimental verification, he inquired whether the dissimilative process itself did not carry the conditions of its own depression. This inquiry led to the refractory phase hypothesis. It involved a re-interpretation of the available data and resulted in a long series of brilliant experimental investigations which reached their climax in the work of Frölich in Germany, of Keith Lucas and Adrian in England, and of Forbes in America.

The hypothesis that inhibition is due to refractory phase is not without its difficulties. Some critics find apparently insuperable objections to it. Many doubt that it accounts for all the phenomena of inhibition. The experimental evidence, however, for some intimate connection between refractory phase and inhibition cannot be lightly dismissed.

Nature of the Refractory Phase.—Refractory phase is a period of decreased excitability subsequent to stimulation. It is called absolute while the re-excitation of a tissue is not possible by maximal stimuli. It is called relative while a

[1] 'Allgemeine Physiologie,' Jena, 1909, Ed. 5, p. 605.

response is evoked only by abnormally intense stimuli or while the response to a given stimulus is abnormally small.

Refractory phase, absolute or relative, appears to be a wide-spread consequence of the reaction of irritable tissue. First discovered in heart muscle,[2] it has been demonstrated in a number of reflexes, in muscle, glands, and nerve fibers. In short it seems to have been found wherever it was sought with adequate techniques. While the question of its biochemical nature is still only partially answered, the theoretical probability of its universality is so strong as to create a presumption that it will be found in tissues where it has not yet been demonstrated.

In general it may be regarded as a period during which there is a re-establishment of equilibrium, disturbed by some previous stimulation. In simple structures, like isolated nerve, muscle fibers, and some of the reflexes, equilibrium is re-established rapidly and the refractory phase ranges in duration in such tissues from a few thousandths of a second in nerve fibers to a few seconds in reflexes. More complicated systems re-establish their normal equilibrium less quickly. It is doubtful if the nervous system as a whole ever returns exactly to any previous condition. Certainly reverberations of any intense stimulation may be traced in mental life long after the original stimulus has ceased and an exact re-establishment of a previous mental condition is so rare that its probability is negligible.

Evidence.—The evidence for the identification of inhibition and refractory phase is partly theoretical and partly experimental. It began with the assumption that refractory phase is a general phenomenon of living tissue and that it may be prolonged indefinitely in complex neural paths by stimuli which succeed one another so rapidly that the path fails to recover normal excitability. What was first found in the strychninized frog by Verworn, Tiedemann, Frölich, Vészi, and others proved to be true of a considerable variety of processes under various conditions.

[2] References to the literature on refractory phase were given in the first paper of this series: Psychol. Rev., 1926, 33, 6.

Historically the first experimental evidence for the refractory phase hypothesis was the so-called 'apparent inhibition' of Wedensky.[3] In connection with a study of the fatigue of nerve fibers he discovered in 1884 the interesting and suggestive 'paradox' that if the nerve of a nerve-muscle preparation is excited by an interrupted induced current, an apparent inhibition takes place when the rapidity of interruption is increased beyond a critical point. This critical point he found to be lowered by narcosis (1903).[4] Hofmann[5] (1902) held that the phenomenon is due to fatigue of nerve end-organs in the muscle.

One of the most frequently cited supports of the refractory phase hypothesis is the extremely condensed paper by Vészi,[6] (1910). His main question was entirely foreign to the problem of inhibition. He sought evidence that the reflex arc in the frog is composed of three neural links. To this end he stimulated the 7th, 8th, 9th, and 10th roots of the ischiadicus separately and in various pairs, using the prepared gastrocnemius as indicator. Only those consequences bearing on the theory of inhibition need now concern us. He found that muscular response was most vigorous from stimulation of the 9th root; least, from the 7th. Stimulation of the latter, however, inhibited reaction from the former, at intensities that were too weak to evoke muscle contractions. Both the 7th and 8th roots also inhibited the 10th. He interpreted this in terms of decrement. That is to say, stimuli reach the final common path through some neural avenues so weakened that they regularly or exclusively evoke inhibition instead of excitation. The mechanism of this decrement was not discussed; neither, as Howell[7] points out, was the possibility considered of specific inhibitory fibres in the 7th and 8th roots.

More direct evidence for the refractory phase hypothesis of inhibition was furnished by Frölich in a series of papers beginning in 1907.[8] Frölich identified the Wedensky paradox

[3] *Pflüger's Arch. f. d. ges. Physiol.*, 1885, 37, 69.
[4] *Pflüger's Arch. f. d. ges. Physiol.*, 1903, 100, 1.
[5] *Pflüger's Arch. f. d. ges. Physiol.*, 1902, 91, 425.
[6] *Zsch. f. allg. Physiol.*, 1910, 11, 168.
[7] *Physiol. Revs.*, 1925, 5, 161.
[8] *Zsch. f. allg. Physiol.*, 1907, 7, 393, 444.

with the relative fatigue of receptive substance for weak stimuli. The experimental evidence was obtained from the reactions of crab claws and the reflex frog. In 1908 he systemized the available data into a 'General Theory of Tonus, Facilitation, and Inhibition.'[9]

According to Frölich relative refractory phase inhibition showed itself in the closing muscles of crabs' claws by beginning tetanus when the muscle stimulated was without tonus. This beginning tetanus was explained as follows: During the first moment of stimulation the refractory phase was shorter than the interval between stimuli. As stimulation continued, relative fatigue set in; the vital processes became slower until the duration of the relative refractory phase came to exceed the interval between stimuli, when inhibition ensued. The same strength and frequency of stimuli which produced beginning tetanus and subsequent inhibition in muscle without tonus produced either inhibition exclusively, or momentary contraction followed by inhibition, when the muscle was tonically contracted. Analogous phenomena occurred in the reflex frog. Low frequency and high intensity of stimulation tended to evoke reflex tetanus. High frequency and low intensity tended to evoke reflex beginning tetanus and subsequent inhibition. Stimuli which were too faint to produce reflex contraction or only slight contraction tended to inhibit an existing excitation.

Frölich regarded these instances of inhibition as a kind of dissimilative paralysis depending on the easy fatigability of the centers. This explained the propagation of inhibitive impulses. According to Frölich they are nothing but dissimilative impulses produced by stimuli of low intensity. According to this theory there is no necessity for assuming the propagation of a specific kind of excitation or the existence of specific inhibitory nerves. The existence of nerve fibres whose main function is inhibition is, however, not excluded. They may be neurones of especially high threshold and fatigability. The general tendency of faint stimuli to produce inhibition and the corresponding long duration of the relative

[9] *Zsch. f. allg. Physiol.*, 1909, **9**, 55.

refractory phase after faint stimuli, Frölich referred to the characteristics of the curve of the restitution of equilibrium as determined by Nernst. Excitability for a given intensity of stimulation should theoretically be re-established more slowly the nearer the logarithmic restitution-curve approaches its limit.

The general relationship between inhibition and the frequency of stimulation was confirmed by Beritoff.[10] He found that the semi-tendinosus contracted reflexly to a stimulation of the ipsilateral peroneal at a rate of 200 per second. During reflex contraction it was inhibited by the additional stimulation of the contralateral sciatic at a rate of twenty-five per second. Action current records showed a series of gaps in the effects of rapid stimulation at the same rate as the slower stimulation. This phenomenon was confirmed by Adrian.[11]

While the experimental study of the relationship between refractory phase and peripheral inhibition began in the Verworn School, it achieved its highest technical refinement in the work of Keith Lucas, Adrian, and Forbes. The Verworn-Frölich doctrine was notably modified by the researches of Keith Lucas. In his investigation Lucas set out from the following critical consideration, "The refractory phase is a consequence of the nervous impulse and if the second of two stimuli falls in the refractory phase of the first and consequently is ineffective in the sense of setting up no nervous stimulus, then it will also set up no refractory phase and a third impulse will again set up a nervous impulse." [12] This was experimentally confirmed by test on the sartorius muscle. "A second stimulus so timed as to fall just within the refractory phase of its predecessor, and consequently to cause no second electrical response in the muscle, does not reduce the electrical response which falls just outside the same refractory phase." The results proved to be quite different on stimulation of the motor nerve in a nerve-muscle preparation. A stimulus "interpolated at such a time that it does set up a secondary impulse in the nerve but is too early to cause response of the muscle"

[10] *Zsch. f. Biol.*, 1924, **80**, 171.
[11] *Brain*, 1924, **47**, 399.
[12] Lucas, K., 'The Conduction of the Nervous Impulse,' Ld., 1917, 86.

prevents a response from the following impulse. His explanation is that the second impulse falls at such a time that it is propagated in the nerve in a reduced condition as though it had passed through a region of decrement.

Keith Lucas' explanation of refractory phase inhibition consequently differs from that of Frölich and Hofmann. It makes inhibition due not to the refractory phase of junctional tissue as Hofmann held nor to the prolonged refractory phase of a narcotized or fatigued region as Frölich held, but to the combined refractory phase of normal nerve and the decrement of junctional tissue.

The Relation Between Summation and Inhibition.—As Adrian [13] put it, "The whole mechanism of peripheral summation and inhibition may be summed up in a very few words. If there is an obstacle to conduction in the course of the nerve fiber this obstacle may be surmounted by timing a series of impulses, so that all but the first are greater than normal, and it may become insuperable if the impulses are timed so that all but the first are smaller than normal." The regular locus of decrement and the normal obstacle to conduction was regarded as the synapse.

The evidence that the synapse is always a region of decrement seems to me to be far from complete. If the synapses are to be regarded as semi-permeable membranes interposed as resistances between conductors of neural energy, it is difficult to understand the fact that neural responses may increase in magnitude within neural tissue as well as decrease. Such increase has been repeatedly demonstrated as in the case of the strychninized spinal preparation. It seems to be peculiarly prevalent in mental life. Dynamically the phenomena of summation would be much the same if neural action across the synapse should prove to be, as many neurologists believe, not conduction in any proper sense of the word but the stimulation of new links in a chain of irritable neurones. The response of any link will obviously depend on characteristics of the link. It may be more or less than that of its stimulator.

[13] *Brain*, 1918, 41, 23.

Adrian distinguished two types of summation of stimuli, namely, summation of local disturbances in the receptor and a summation of nervous impulses. In the former, a stimulus which is too weak to produce a nervous impulse may produce local disturbances which take a short time to subside. "And if another stimulus of the same strength is applied before it has subsided, the additional disturbance may be great enough to reach the intensity required to start the nervous impulse." This effect is 'purely local' and is generally known as the 'summation of inadequate stimuli.'

Summation of nervous impulses is quite a different matter. It depends on secondary stimulation during a phase of heightened neural activity such as occurs as the end effect of previous stimulation. The consequent normal excitation is then stronger than normal as is indicated by its ability to travel further through a region of decrement. If the synapses are to be regarded as regions of decrement, as Adrian holds, then the summated neural excitation will be able to pass synapses which would otherwise be impassable, to stimulate a larger number of motor neurones and to increase the consequent response.

The converse would probably hold true if the secondary stimulation reached the nerve fiber in a phase of depressed neural activity, *i.e.*, during its relative refractory phase. In that case synapses normally conducting with a decrement would become entirely non-conducting and previous action would be inhibited.

Both of Adrian's two kinds of summation might combine to produce increased response, since summated conductions in the stimulated neurones would not only excite those neurones which were normally susceptible but the repetition of subthreshold local disturbances in other receptor neurones might also summate, provided each threshold stimulus fell within the phase of increased irritability produced by the preceding one. Conversely if the stimulating impulses were so timed that each succeeding stimulus fell within a phase of depressed local irritability, no response would be elicited in receptor neurones with a high threshold.

Whether the synapse is a point of resistance to a neural flux or a point of re-stimulation of neural links, any normal series of excitatory impulses may, and probably will, develop local summation in some receptor neurones and local inhibition in others, which have a slightly higher threshold, or a longer refractory phase. Every normal response may consequently be regarded as a balanced or combined response with both summated and inhibited factors.

If this construction is true, it should have far-reaching consequences in the psycho-physical understanding of mental events. The depression of irritability by any means whatsoever should theoretically be found to depress the balance between excitation and inhibition. Whenever the accumulation of fatigue-stuffs or of depressing glandular secretions reaches sufficient concentration, the inhibitory effects of stimulation would overbalance the summation effects. In extreme cases of accumulation sleep would be a normal consequence. The spread of inhibiting excitations through delay and association paths would under such circumstances appear as though it were a spread of inhibition. Moreover, the same mechanism would tend to develop more or less stable systems of inhibition and stimulation throughout the nervous system. Whenever receptor thresholds to specific excitors are relatively high, one would expect inhibition. Conversely, whenever thresholds are relatively low to the same excitors, one would expect summation. Such an interplay of summation and inhibition consequent to every stimulation might easily occasion the illusion of lawlessness that seems to characterize so much of our mental life.

Critique of the Refractory Phase Hypothesis.—Verworn saw very clearly certain discrepancies between the all-or-nothing law and his formulation of the refractory phase hypothesis. According to the law any element of the nervous system in so far as its action involves the nerve fibers responds to stimulation maximally or not at all. The experimental evidence for this law is held to be conclusive for motor neurones with a strong presumption for sensory ones. There are real discrepancies between the law and the refractory phase hypothesis.

In the first place inhibition is a more or less protracted process. If it is really conditioned by the refractory phase, then the latter must be continually renewed to produce prolonged inhibition. But refractory phase is the result of activity; if the all-or-nothing law holds it is the result of maximum activity. Consequently the paradox seems to follow, that the protracted inhibition of a tissue is coincident with maximum activity. This paradox appears to be a fundamental difficulty of the Verworn formulation of the hypothesis and constitutes one of the main grounds for the revision by Lucas and Adrian. Experimentally Adrian found that the excitation which was evoked in a nerve fiber by a stimulation occurring at the close of refractory phase was not decreased by an interpolated stimulus unless the latter evoked a response.

The Lucas-Adrian hypothesis of what happens in a chain of neurones has already been stated. It may be recapitulated as follows: The decreased neural excitation which occurs during the relative refractory phase of a nerve suffers extinction at a synaptic region of decrement. It can hardly be held that this is a satisfactory explanation of all reflex inhibition, since the refractory phase of nerve does not last over 0.01″. The refractory phase of all the reflexes is much longer. If, as Hofmann held, the synapse is also the seat of a refractory phase, one faces anew the theoretical discrepancy between the maintenance of a refractory phase and inhibition, provided the former is always a consequence of action.

The theory of refractory phase inhibition following action meets further difficulties when applied to the sequence of events either in the reciprocal inhibition of antagonistics or in the mental life. If inhibition requires preliminary action, the inhibition of extensor muscle fibers on stimulation of a flexor would be possible only after their previous contraction. This condition has never been reported experimentally. Of course this negative evidence is not conclusive since attention has not been called to the matter. Moreover, in some cases at least, reciprocal innervation fails to reach its maximum efficiency at its onset; for example, in rapid oscillatory movements of the finger, the first movements are slower and of less

amplitude than the succeeding ones. Records of finger movements which were taken with a very sensitive microscope recorder in pneumatic connection with a sphygmographic cuff around the wrist show four contraction or relaxation episodes for every complete oscillation except the first. While these episodes come to be exceedingly regular after the first few, the latter are quite atypical of the rest. The record of the first oscillation is relatively long and smooth. That of the next one or two still shows unusual relationships between the several episodes. The phenomenon deserves careful analysis.

Considered in terms of the mental life the requirement that positive action must precede inhibition would mean that the inhibition of an act could only occur after the act was an accomplished fact. Reactions could never be blocked before they occurred. This seems to be contrary to fact in the anticipatory checking of undesired reactions and in the deadening of the thigh by which it becomes less sensitive to reflex stimulation as described in the initial paper of this series.[14] Either refractory phase inhibition must on occasion be elicited by subthreshold stimulation or there must be some other mechanism of inhibition. The ordinary progress of mental events requires a form of inhibition which may on occasion operate without previous effective stimulation. Without such a mechanism irrelevant matter could never be excluded by attention unless it came to consciousness first. Previous consciousness of the inhibited factors is apparently necessary neither for attention nor recall. It is certainly not always necessary in voluntary reactions as is demonstrated by the delay or block of an adequate reaction after a false start. As Howell [15] puts it, "It would seem inadequate to define inhibition simply as the quelling or blocking of action in progress. It is something more positive and characteristic."

A second difficulty in the refractory phase hypothesis as pointed out by Forbes [16] concerns the alternation of inhibition and excitation which he located in the 'premotor' neurone producing inhibition with stimulation frequencies above a

[14] Psychol. Rev., 1926, 33, 8.
[15] *Physiol. Revs.*, 1925, 5, 166.
[16] *Quar. J. Exper. Physiol.*, 1912, 5, 149.

critical frequency and excitation below it. After the critical frequency has been reached or rather after the onset of inhibition, he found it difficult to explain on the basis of the refractory phase hypothesis how any additional stimulation whatsoever can re-establish excitation. The experimental facts prove that it does. Sherrington showed that excitatory stimuli may break through a simultaneous inhibition and cause muscular contraction. It is possible that this discrepancy between the facts and the theory is not as great as at first it seems to be. The facts would not be unexplainable from the standpoint of the hypothesis, if it were taken into consideration that refractory phase is not always absolute and does not necessarily involve all the elements of a neuromuscular system to the same extent. We have already discussed the possibility that in harmony with the all-or-nothing law local summation and inhibition may depend on the strength of stimuli that reach the relevant tissues as well as on their frequency. There seems to be experimental evidence that refractory phase inhibition depends on the intensity of stimuli as well as on their frequency.

A third difficulty is mentioned by Howell [17] in his searching criticism of the refractory phase hypothesis of inhibition. He points out that according to the all-or-nothing law the graduation of response subsequent to the absolute refractory phase as the latter gives place to the relative refractory phase is difficult to understand. This seems to be peculiarly true if we accept the Lucas and Adrian theory of extinction of conduction in a region of decrement. It seems to me, however, that this is no more incomprehensible than the graduation of response in general. The latter is quite as well founded as the all-or nothing law. The facts of graduation of response even in the reflexes must be accepted as demonstrated. It is not, however, a question of accepting one or the other. The validity of the all-or-nothing law can be maintained by amendments or limitations which allow for the apparent inconsistency. Both may be found consistent if due weight is given to the difference of threshold between elements of the same tissue

[17] *Loc. cit.*

and the differences in the extent and duration of the relative refractory phase.

Howell points out still another difficulty of the refractory phase hypothesis in that it furnishes no mechanism for the conduction of inhibition from one part of the nervous system to another. In this it seems to be in direct opposition to the published conclusions of Pawlov. But as we have already pointed out, the apparent conduction of inhibition may be due to the propagation of stimuli which are too faint at a given frequency to summate but not too faint to produce and maintain locally depressed irritability. Doubtless, moreover, the hypothesis of a relative refractory phase inhibition cannot be regarded as ultimate. Refractory phase itself is no final or irreducible physiological phenomenon. Following the metabolic process one step further back, one may ask, 'What is the cause of the refractory phase'? Frölich identified refractory phase with relative fatigue. That is to say, it is a state consequent to excitation in which stimuli of the same order fail to evoke a reaction of the same amplitude. In refractory phase the electro-chemical or metabolic equilibrium has not yet returned to normal. If this theory of the refractory phase is valid, then the conditions of relative fatigue are the ultimate conditions of inhibition, whether these are CO_2, fatigue toxins, humors, or secretions of the ductless glands. The ultimate problem of inhibition is not solved by referring it to the refractory phase. That is merely a convenient stopping place for the reformulation of experimental questions. If our analysis holds it really amounts to saying that the conditions of inhibition are any physiological conditions that decrease irritability or raise the thresholds of effective stimulation at any given frequency for any link in the neuro-muscular system. Any condition of decrement may become a condition of inhibition. This in no way libels the utility of the refractory phase hypothesis. The latter emphasizes what may be and probably is a common class of decrements whose widespread distribution in excitable tissue warrant our treating it as a species.

Evidence of Refractory Phase Inhibition in Man.—Since it lacks both the information and the techniques to acquire in-

formation, psychology has no license to take sides on the purely physiological questions which are raised by the refractory phase hypothesis. The ultimate nature of refractory phase inhibition, its relationship to the all-or-nothing law, and the locus of its incidence must be settled by methods over which the psychologist has no mastery. Quite aside from these physiological questions, however, there is a legitimate psychological problem in the experimental question whether or not there is a kind of inhibition in human behavior which corresponds to refractory phase, and how far refractory phase can explain the outcrop of inhibition with which we are familiar. These questions are not to be decided by the congruence or lack of congruence of the hypothesis with more or less probable physiological laws. The real test of the refractory phase hypothesis in human life is its congruence with experimental facts.

Is there any experimental evidence that refractory phase actually operates to inhibit any form of human behavior? In the human reflexes at least this must be answered in the affirmative. Not only has refractory phase been experimentally demonstrated in all human reflexes in which it has been sought by available techniques but in one case at least, namely that of the knee-jerk it has been shown to operate in a protracted series of stimulations to inhibit all reactions after the first.[18] Quite apart from the question of mechanism by which a refractory phase can be continued without overt acts, the fact remains that it may be so continued. In the case under consideration the sequence of stimuli was too slow to have produced an effective refractory phase in nerve fibers. The experimental results consequently cannot be referred to Keith Lucas' formula of extinction at the synapse of reduced nerve conduction. One suspects rather a local condition of depressed irritability at some point of restimulation in the neural chain. This was suggested by Keith Lucas' discrimination between local and nerve summation. While detailed experimental investigation of the locus of the conditions of this decrement seems to be outside our present resources, the fact

[18] *Zsch. f. allg. Physiol.*, 1910, 12, 54.

remains that under certain experimental conditions refractory phase inhibition is a real phenomenon of human behavior. The general extension of the principle to the more familiar outcrops of inhibition faces the objection we have already outlined, namely: There is often no noticeable overt action at the beginning of inhibition, which is supposed to be necessary to produce a refractory phase. This seems to be a critical point in the application of the refractory phase hypothesis to human experience and behavior.

The crucial experimental question consequently seems to be, not whether a refractory phase inhibition is possible in human life, but whether refractory phase inhibition can be induced on occasion without an antecedent overt act. In terms of our previous supposition this question might be expressed as follows: Can the local depression of irritability at any point of neural restimulation prevent response to a protracted series of stimulations without a preliminary overt act? In other words, can stimuli which are too weak to produce reaction depress the irritability of a neural link so as to produce inhibition?

There is some evidence in the literature of the subject that would lead us to expect a positive experimental answer to this question. There seems to be no doubt that stimuli which are too weak to evoke muscular contraction have some effect on irritability. This is proven by the summation of inadequate stimuli. Where readiness to react can be produced by stimuli which are too faint to evoke positive reaction, there is no theoretical ground to exclude the possibility of evoking unreadiness by stimuli of a similar order.

The tendency of weak stimuli to produce inhibition has been reported from various sources. Wundt [19], reporting on the excitation and inhibition produced by the constant current at the cathode and anode respectively, found that very light stimuli caused anode inhibition alone while stronger stimuli produced equal inhibition and excitation at the respective poles. Verworn [20] reporting on the movements of the cilia of

[19] 'Untersuchungen zur Mechanik der Nerven und Nervcentren,' Erlangen, 1871.
[20] 'Irritability,' New Haven, p. 191.

paramecia found that the movements of the cilia are inhibited when the body of the paramecium meets a faint mechanical resistance. Their movements are reversed into a negative thygmotaxis when the body meets a strong resistance. A more extensive account of the phenomena consequent to weak stimulation was given by Miss Buchanan.[21] "The electrical reflex responses of muscles which serve as extensors, especially those which serve mainly as such, may begin with a transient positive variation, usually lasting for about 0.01 sec. when the stimulus is a single instantaneous one." It occurred in more than 50 percent. of the cases, 'when there is no reason to suspect any abnormal condition of tonus,' in response to stimulation of both the crossed brachial and that of the same side, when any one of three drugs, strychnine, phenol, or caffein, had been used to increase the excitability of the cord. "With two preparations in which the positive variation was found to last over 0.04 sec. the mechanical responses (of the gastrocnemius) began with, or was mainly relaxation instead of contraction." Interruptions of the action current in antagonistic muscles indicated that reciprocal innervation was in force even when this could not be detected in the mechanical response of muscle.

Frölich[22] found that with gradually increasing strength of stimuli there was a zone in which they failed to evoke a muscular response. They might be strong enough to inhibit claw openers while they were too weak to excite them. This confirmed Biedermann's contention for a neutral zone which can be produced by variations in intensity as well as by variations in frequency. Higher frequency at a strength which was just sufficient to stimulate the openers and inhibit closers operated to inhibit the openers. After that frequency was reached the stimuli could be observably increased before the closers were stimulated to action. By increasing the frequency the limiting strength of inhibiting stimuli was pushed upward. Sherrington found that strychnine, which notably increased the irritability of tissue, transformed inhi-

[21] *Quar. J. Exper. Physiol.*, 1912, 5, 128.
[22] *Zsch. f. allg. Physiol.*, 1907, 7, 393.

bition into excitation. Apropos to this Frölich remarks, "Inhibition is nothing but weak excitation."

The relaxing effect of faint or moderate stimuli as contrasted with the exciting effect of strong stimuli is obvious in many of the human sensory fields. Moderate warmth is relaxing and ennervating, while hot things and cold things stimulate and may lead to violent reaction. Light, contacts, smells, and tastes apparently follow the same rule.

Just as strychnine increases the effectiveness of stimuli, changing inhibition to excitation, so chloroform converts exciting into inhibiting stimuli. Bayliss reported [23] that when electrodes were placed on the floor of the fourth ventricle of the so-called vasomotor center of the rabbit, chloroform converted the usual pressor effect into a depressor one.

Experimental Test of Inhibition without Antecedent Overt Act in Intact Human Nervous System.—Direct experimental attack on the question whether refractory phase can be developed in humans without antecedent overt act seemed entirely feasible. In terms of the knee-jerk and the lid reflex the question would take the form: "Can refractoriness be produced by a stimulus which is too weak to evoke a recordable reflex so that subsequent normally superthreshold stimuli become ineffective?" The main requirements seemed to be a series of graduated stimuli from subthreshold to normally effective and adequate recording techniques. Adequate techniques were already available for both the knee-jerk and the lid reflex.[24]

In our experiments the knee-jerk was recorded from isometric muscle by the registration of muscle thickening. The quadriceps-proprioceptors were stimulated by two weak and one stronger blow on the patellar tendon. The blows were delivered by three pendulums 20 cm. in length and of appropriate weight. The aim was to make the first two stimuli at or near the threshold and the last clearly superthreshold. The pendulums all fell from the same height. By interposing a light horizontal transmitting rod between the pendulums

[23] *Roy. Soc. Proc.*, 1908, B 80, 339.

[24] *Zsch. f. allg. Physiol.*, 1910, 12, 1. Dodge and Benedict, 'Psychological Effects of Alcohol,' 1915, 35 *ff*.

and the tendons, the blows were transmitted to the tendon at the same point. Plate I shows a series of records made from my own patellar reflex. The pendulum weights were respectively 2, 2, and $3\frac{1}{2}$ ounces. Each fell 30 degrees to the vertical and the interval between stimuli was 3/100 of a second. All records of this series were preceded and succeeded at an interval well outside the refractory phase by stimuli from the heavy pendulum. The recorded amplitude of these control reflexes ranged from 1/8 inch to 3/4 inches. It was never zero. The weaker stimuli of the series were not always subthreshold and the reactions to the superthreshold stimulus vary within considerable limits. Both were subject to reinforcements and inhibitions of which we never had an adequate picture. In the case of the last record there was disturbing conversation in the room. This was noted on the record as a suggestion of the cause of an enormous exaggeration. The records are obvious. When a weak stimulus evoked a response, the third or stronger stimulus regularly found the reflex system in a refractory condition and the response was regularly smaller than that evoked by isolated stimuli from the same pendulum.

FIG. 1

In record line No. 7 (reading down) the response to the third stimuli had almost disappeared without evidence of an overt act in the preliminary record that can be differentiated from

the mechanical vibration of the muscle. In line No. 5 the response to the third stimulus completely disappeared without any true antecedent contraction. In both the other subjects, E and K, the knee-jerk was more difficult to elicit. All the stimuli were consequently increased in strength to an extent which made the records difficult to interpret on account of larger mechanical vibrations of the muscle consequent to the blows. But apparently there were numerous instances for both subjects of the total disappearance of all overt muscular contractions in consequence of a series of stimuli which was initiated by a subthreshold stimulus.

Similar results were found for the lid reflex to sound stimuli though there was a reversal in the relative sensitivity of the subjects. For both subjects, E and K, no noise that our instrument could produce was so faint as not to be followed by a beginning wink. My own reactions are given seriatim in the following table:

TABLE I

MILLIMETERS OF LID MOVEMENT IN RESPONSE TO SOUND STIMULI, AS RECORDED BY AN ARTIFICIAL EYELASH. SUBJECT D[25]

	Stronger Stimuli	Weaker S'	Stronger S''
1	7.5		
2		3.0	0.5
3		0	0
4	2.0		
5		1.5	0.2
6		0.3	0.1
7	4.0		
8	4.0		
9		1.0	1.0
10		0.2	0.5
11		3.0	1.0
12	2.0		
13		2.0	0.5
14		3.0	1.0
15	3.5		
16		3.0	0.2
17		1.0	0.2
18	3.5		
Mean	3.78 mm.	1.64 mm.	0.47 mm.

[25] These records were taken at the Psychological Laboratory of Wesleyan University. It is a pleasure to record the devoted assistance of the members of my class in Experimental Psychology. My thanks are especially due to Mr. A. A. Elsey and Mr. W. I. Keith.

The data are reproduced from shadow records of great delicacy and complete freedom from recorder friction. The experiment was conducted so that control experiments were interspersed in the series registering the reaction to the stronger stimulus alone. The results of these control experiments are given in the column to the left. In no case either in the control experiment or in a long series of records lasting over several years did the single stronger stimulus alone fail to evoke a measurable wink. Their average amplitude in this experiment was 3.8 mm. When a weaker stimulus preceded the stronger the average amplitude of reaction to the latter was reduced to less than .5 mm. The responses were sometimes barely perceptible. In one case the reaction to both stimuli was zero. Workers in the Institute of Psychology have recently obtained similar results using the guinea pig as subject.

It proved difficult to produce stimuli too weak to elicit perceptible reactions without gross change in the quality of the noise. With our delicate recording techniques reactions were registered from surprisingly weak stimuli.

The results are essentially similar in both the knee-jerk and the lid-reflex. Both reflexes suffer marked decrement in consequence of nearly threshold stimuli. Both reflexes show evidence of a mechanism which may on occasion inhibit all response after a preceding weak stimulation which failed to evoke an overt act. The agreement of both reflexes enormously increases the reliability of the results. Even in those cases where the second response was not totally inhibited it seems probable, according to the all-or-nothing law, that neuromuscular elements which did not react maximally to the preliminary weak stimuli participated in the refractory phase that it set up. The gross decrease in total response to the stronger stimuli seems otherwise inexplicable. With less delicate recording devices most of the responses subsequent to weak stimulation would have been overlooked. Just where the neural action ceased to be an adequate stimulus to the next link in the neuromuscular chain is utterly unknown.

Conclusion.—The experiments whose results are here re-

ported make no pretense of finality. They do not demonstrate the validity of the refractory phase hypothesis. That seems already to have been done. Our records add little to the mass of evidence already available. They do show, however, that under certain favorable but unknown circumstances stimuli which are too faint to evoke recordable muscular contractions may on occasion completely inhibit the usual or normal response. They show that the refractory phase may produce in the intact human central nervous system phenomena of inhibition of a kind with which psychology is familiar. That is to say, refractory phase may produce inhibition without introductory overt acts.

The further significance of these experiments and theoretical considerations it seems to me is to indicate the probable importance of systematic investigation of the effects of stimuli which are too faint to evoke overt response. Somehow, somewhere, they do something which does not conform to the all-or-nothing law of conduction. As to what that something is we are profoundly ignorant. No one questions the importance of the summation effects. I conjecture that the rôle of the refractory phase will also be found to be of considerable value in developing a future theory of behavior as well as in the practical control of reaction. One suspects that it is involved in that catharsis by which the imaginary facing of a crisis particularly in artistic form prepares us to face the actual crises of life with less perturbation. I conjecture that it is a condition of training by which the effect of any naturally disturbing stimulus may be moderated by the proper graduation of preliminary experience. The firing pointer in the Navy is not assigned at first to guns of large calibre. His early training is graduated to inhibit disturbing reactions. It begins with the dotter and subtarget practice. The full horrors of warfare are not presented at once to the raw recruit. The enormities of style are developed by graduations that prepare the way for the extremes and prevent mob interference which not infrequently follows sudden social innovations. The same principle may apply to the development of a fugue and the plot of a tragedy, to the presentation of a

scientific thesis, the patter of a salesman, and the evolution of a social order. I predict that it will be found to be involved in the theory and practice of immunity.

The rôle of consciousness in human behavior has often been the subject of controversy. In addition to being a special variety of integration and a convenient substitute for overt acts in the trial and error method of meeting a new situation, it may also, through imagination or memory, represent an abortive act in the development of inhibition. I believe that this is an important factor in the psychology of punishment and the elimination of undesired responses.

Summary.—The hypothesis that inhibition is a prolonged refractory phase originated with Verworn. It was modified by Keith Lucas to include the decrement of junctional tissue.

Junctional tissue seems also on occasion to condition an increment. Whether successive stimuli summate or inhibit each other depends on their intensity and frequency and on the latency, threshold, and refractory phase of the several receptor elements. Both inhibition and summation may occur simultaneously in the different elements of the same neural link in response to the same stimulus.

Various conflicts have been found between the hypothesis and physiological generalizations. The most serious of these conflicts is with the all-or-nothing law. It apparently leads to the paradox that protracted refractory phase inhibition must be periodically renewed in some neuromuscular element by maximal activity.

The assumption that antecedent overt response is necessary for inhibition would contravene many well documented observations. There is experimental evidence that refractory phase may be protracted without overt activity. Crucial experiments tend to show that refractory phase inhibition may on occasion be produced by stimuli which fail to produce any antecedent overt act. This has important consequences in psychological theory and in the control of behavior.

SIR CHARLES BELL: A CONTRIBUTION TO THE HISTORY OF PHYSIOLOGICAL PSYCHOLOGY

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Physiological psychology, while basic to a genuine understanding of mental life, is itself a discipline founded in the older natural sciences. Historically considered, astronomy, physics, chemistry, and above all the biological and medical sciences were propædeutic to a true physiological psychology. Ernst Heinrich Weber, Johannes Müller, Gustav Theodor Fechner, and Hermann von Helmholtz are among those who have been rightly credited with an important share in establishing both the method and the factual content of the young science of psychology. On the other hand, the contributions of the eminent Scottish surgeon and physiologist, Sir Charles Bell, to the groundwork of the science of mental phenomena have never been given the recognition which they merit. It is the purpose of the present paper to present a brief critical and historical account of that part of Bell's work which touches upon such topics as are now generally included within the systematic scope of physiological psychology.

I

Biographical Introduction

Born in Edinburgh in 1774, Charles Bell was the youngest son of a respected but impecunious minister in the Episcopal Church of Scotland.[1] His general education was informal:

[1] No really satisfactory biography of Sir Charles Bell exists. The biographical sketch given in this paper is based largely upon the following sources: 'Letters of Sir Charles Bell, Selected from His Correspondence with His Brother George Joseph Bell,' 1870 (George Joseph Bell was Professor of the Law of Scotland in the University of Edinburgh); 'Sir Charles Bell,' an unsigned article in the *Quar. Rev.*, 1843, **72**, pp. 192–231; M'Neill, Sir J., 'Biographical Notice of the Late Sir Charles Bell, K. H.,' *Trans. Roy. Soc. Edinburgh*, 1843, **15**, pp. 397–408; Pichot, A., 'Sir Charles Bell; Histoire de sa vie et de ses travaux,' Paris, 1858, translated as 'The Life and Labours

'My mother,' he says, 'was my only teacher.'[2] Throughout life he complained of a poor rote memory for facts that he could not reason upon. In youth the mastery of the subtleties of Latin syntax were so difficult for him that he was considered dull. Charles' elder brother, John Bell, early gained a place as one of the first surgeons of Edinburgh and it was largely from him that the younger son learned anatomy and surgery. He also attended lectures in the University of Edinburgh. In dissection he showed marked ability from the first. His success in medical study finally eradicated the memory of his difficulties with Latin. 'It was only,' he tells us, 'when in professional education I found subjects more suited to my capacity that I began to respect myself.'[3]

Always, throughout life, he delighted in mechanical or artistic work with his hands. Under the direction of a well-known Scotch artist the boy was given encouragement in painting and drawing, exercises which continued until the very year of his death. His own illustrations in his books show that he achieved genuine success in this field.[4] In later life he added modeling to his accomplishments.

As a mere youth Charles Bell won recognition in the dissecting room and at the operating table. Before he had completed his student years he had written upon anatomy.[5] While still in his twenties he was a recognized lecturer and a fellow of the Royal Society in Edinburgh. Controversies

of Sir Charles Bell,' London, 1860; Moore, N., 'Sir Charles Bell,' *Dictionary of National Biography*, 1885, **4**, pp. 154–157; 'Charles Bell,' *Nouvelle Biographie Universelle*, 1853, **5**, pp. 211 *f.*; 'Sir Charles Bell,' *The Encyclopaedia Britannica* (11th. Ed.), **3**, pp. 284 *f.*; Williams, H. S., 'The Century's Progress in Experimental Psychology' (with portrait of Sir Charles Bell), *Harp. Mag.*, 1899, **99**, pp. 512–527; Gamgee, S., 'Sir Charles Bell and Sir James Simpson: A Biographical Study' (with bibliography of Bell's writings), *Birmingham Med. Rev.*, 1875, **4**, pp. 85–100; Howe, A. J., 'A Sketch of Sir Charles Bell' in 'Miscellaneous Papers,' 1894; and Ebstein, E., 'Einleitung,' pp. 3–11 in 'Charles Bell: Idee einer neuen Hirnanatomie, Originaltext und Übersetzung,' Leipzig, 1911. *Cf.*, also, 'The Work of Sir Charles Bell, etc.,' *Brain*, 1926, **48**, pp. 449–457. (Reference is not made to this study, which appeared after the present article was in proof.)

[2] *Op. cit.*, in *Quar. Rev.*, p. 193.

[3] 'Letters,' p. 13.

[4] The writer has had the privilege of viewing two of Sir Charles Bell's original and wholly delightful sketches in water color which adorn the walls of the College of Physicians of Philadelphia. They were the gift of Mr. H. G. Shaw.

[5] Bell, C., 'A System of Dissections,' 1798.

with his brother John's professional rivals so hindered the young scientist that, at the age of thirty, he set out alone for London. Without fortune or influential connections his first years in the metropolis were full of difficulty. His brilliant physiological discoveries and important publications together with his skill as a surgeon and his eloquence as a lecturer,[6] soon, however, won him a place among the leaders of the profession in the capital. After some years of successful lecturing to private classes he took over the direction of the Hunterian School of Medicine in Windmill Street, then a world center for medical education. At the formation of London University this school was discontinued and Bell accepted the chair of physiology in the newly formed institution. London University did not prosper and at length in the hope of securing more leisure for scientific study but at a considerable pecuniary sacrifice, Bell returned to Edinburgh to accept the chair of surgery in the university of his native city.

While in London, Bell received many honors. Beside membership in learned societies he was knighted in 1831 by George IV, and two years later he was given the honorary degree of M.D. by the University of Göttingen. In his travels he was received as 'greater than Harvey.' It is significant of his influence upon scientific thought that his one-time opponent,[7] Baron Cuvier, just before death is reported to have said to those at his bedside, 'This proves the truth of Charles Bell's nervous system—*Ce cont les nerfs de la volonté qui sont malades*.' [8]

Scientific in his thought, Bell usually avoided metaphysical speculation. He had little use for mystical fancy. In one place he affirms that "painters have nothing in their heads but what was put there. There is no power in us 'to disengage ourselves from material things and rise into a sphere of intellectual ideas'. . . ." [9]

[6] *Cf.* for report of an auditor, Gamgee, S., 'Sir Charles Bell and Sir James Simpson,' *op. cit.*, p. 90.

[7] *Cf.* M'Neill, 'Biographical Notice of the Late Sir Charles Bell, K. H.,' *op. cit.*, p. 403.

[8] 'Letters,' p. 314.

[9] Bell, C., 'The Anatomy and Philosophy of Expression as Connected with the Fine Arts' (sixth edition), 1872, p. 23.

During life Sir Charles Bell gained distinction, but not fortune. In 1842 he died, 'as poor as he began—but as spotless: leaving to his widow only the memory of his gentle virtue and the immortality of his name.'[10]

II

Bell's Law of the Structural and Functional Discreteness of the Motor and Sensory Nerves

Of first importance in considering the contributions of Sir Charles Bell to physiological psychology is his fundamental discovery in regard to the nature of the response mechanism. In the long and detailed history of the development of the knowledge of reflex action Bell's name justly occupies a situation of honor. Excellent general accounts of the early history of this system are available.[11] Here it will be necessary to indicate only a few of the outstanding early contributions to this history in order to give some idea of the general state of the science of neurology at the time that Bell's work began.

In antiquity Galen had made observations upon the pupillary reflex, but it was not until the time of Descartes that a significant advance was made in the general conception of the response mechanism. This famous French philosopher and mathematician was the author, as is well known, of a daring speculation upon animal mechanism. He postulated a reflex apparatus, in many respects an adumbration of the facts which have been established by later experimental investigation.[12]

[10] *Op. cit.*, in *Quar. Rev.*, 1843, p. 231.

[11] *Cf.* Eckhard, C., 'Geschichte der Entwickelung der Lehre von den Reflexerscheinungen, III Beiträge zur Geschichte der Experimentalphysiologie des Nervensystems,' *Beiträge z. Anat. u. Physiol., von C. Eckhard*, Giessen, 1881, **9**, pp. 29–192. See also, Hall, G. S., 'A Sketch of the History of Reflex Action,' *Amer. J. Psychol.*, 1890, **3**, pp. 71–86 and a continuation of the same by Hodge, C. F., *op. cit.*, pp. 149–167 and 343–366.

[12] Descartes, R., 'Oeuvres' (edited by Adam and Tannery), Paris, 1909, **11**, pp. 140 *ff*. See also in the same volume the wood-cuts on the pages following p. 708. *Cf.* also, Bayliss, W. M., 'Principles of General Physiology,' 1924, pp. 494 *ff*.; and for a particularly valuable account of Descartes' relation to the general rise of the doctrine of reflex action, see Lecture X (pp. 253–297) in 'Lectures on the History of Physiology,' by Sir Michael Foster, 1901.

Slow was the progress of the actual research which has led to the present knowledge of the reflex mechanism. The naturalist Swammerdamm published in 1757 observations on response in his *Bibel der Natur.*[13] Among the other writers of the eighteenth century who touched upon this subject, the Scottish physician, Robert Whyatt deserves particular mention because of the experimental nature of his work. He was the first experimentalist to make a thorough study of the actions of decerebrate animals. Even after these studies, however, he had but a vague notion of the mechanism of response.[14]

Thus, at the opening of the nineteenth century, notwithstanding experiment and speculation, the fundamental conceptions of the anatomy and physiology of the nervous system were still very obscure. All neural structures were supposed to transmit promiscuously 'the powers of motion and sensation.'[15] The researches of Bell changed all this. He was able to show that the sensory and motor functions were carried on in different sets of nerves. This demonstration was first made on the roots of the spinal nerves. He was eventually able to prove without reasonable doubt that the dorsal or posterior root is concerned with afferent impulses; the ventral or anterior roots with efferent conduction. He further demonstrated that while motor and sensory fibers may appear peripherally to form a single nerve that nevertheless they are always really distinct. Order was thus brought out of confusion; a new guiding principle established; and experimental research in neural physiology stimulated afresh. Johannes Müller, who himself contributed to the fundamental knowledge of the response system, is said to have declared Harvey's discovery of the circulation of the blood and Bell's discovery of the structural and functional discreteness of the motor and sensory nerves to be the two grandest discoveries of physiological science.[16]

[13] Eckhard, C., *op. cit.*, p. 36.

[14] *Cf.* 'The Works of Robert Whyatt, Published by his Son,' 1796, pp. 151 *ff.*

[15] Shaw, A., 'On the Nervous System' (Appendix to the 6th Ed. of Bell's 'The Anatomy and Physiology of Expression'), 1873, p. 233. *Cf.* also Hodge, *op. cit.*, p. 343.

[16] Preface to the 'Letters,' p. 5.

A number of efforts have been made to question the priority of Bell's discoveries. The two most important contenders for this honor were the Scotch physician Alexander Walker and the French physiologist François Magendie.[17] In order to pass judgment upon this question it will be necessary to consider the dates of Bell's early experiments and writings upon the nerves. The first paper of Bell to deal with the functions of the nervous system considered in the new way [18] was a privately printed monograph, 'Idea of a New Anatomy of the Brain: Submitted for the Observations of His Friends.' This booklet is not dated. A study of the records of the printing office where it was set in type shows that the monographs were delivered in 1811.[19] There is little doubt that this work existed in manuscript before this date and that it had at least in part been conceived in 1807.[20]

During the period of these discoveries Bell was lecturing to classes in London. At all times it was his habit to communicate the progress of his experiments orally to his pupils. His friends suggest that this custom may have resulted in the plagiarism by others of the fundamental ideas of his researches.[21] In any case, as early as 1808, or only a few months

[17] A physician by the name of Mayo also claimed the distinction of this discovery, but his pretentions seem so obviously unsound that they will not be considered in this paper. *Cf.* Shaw, A., 'Narrative of the Discoveries of Sir Charles Bell in the Nervous System,' 1839, pp. 3 *ff*.

[18] Charles Bell had already written in Vol. 3 of 'The Anatomy of the Human Body by John and Charles Bell,' a long consideration of the nerves in the traditional manner.

[19] This information is given in a note appended to the reprint of the 'Idea of a New Anatomy of the Brain,' in *J. Anat. & Physiol.*, 1869, 3, p. 166.

[20] This is shown in a letter of Charles Bell to his brother George Joseph Bell dated July 8, 1808. 'Letters,' p. 124. (Part of this letter is also added as a note to the reprint of the 'Idea of a New Anatomy of the Brain,' *op. cit.*, p. 149.) There is evidence to show that as early as 1807 the fundamental idea of the discovery of the separate function of the nerves had already been conceived. He wrote at this time ('Letters,' p. 118): 'I consider the organs of the outward senses as forming a distinct class of nerves from the other.' These he traces to the brain from which 'again the greater mass of the cerebrum sends down processes or crura, which give off all the common nerves of voluntary motion, etc. I establish thus a kind of circulation as it were. In this inquiry I describe many new connections. The whole opens up in a new and simple light; the nerves take a simple arrangement; the parts have appropriate nerves; and the whole accords with the phenomena of pathology . . . .'

[21] *Cf.* the note added by Alexander Shaw to the reprint of the 'Idea of a New Anatomy of the Brain,' *op. cit.*, p. 173.

after the first written record of Bell's research, a certain Alexander Walker published an account of the difference in function between the roots of the spinal nerves.[22] Unfortunately for Walker's claims, however, he asserted that the dorsal root is motor in function and the ventral root sensory.[23] How this error could have been made by one who otherwise claims the fundamental discovery, is difficult to understand, for the demonstration of the motor function of the ventral root is subject to direct observation after the initial dissection has been performed. The correct facts, indeed, upon the nature of the roots had apparently already been determined and delivered in lectures by Bell, for in his short essay on the nerves he writes, "On laying bare the roots of the spinal nerves I found that I could cut across the posterior fasciculus of nerves which took its origin from the posterior portion of the spinal marrow without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of a knife, the muscles of the back were immediately convulsed." [24] This demonstration of the motor function of the ventral root is so direct that its erroneous recording by Walker casts doubt upon all of his subsequent contentions. In spite of Walker's continued attempts to refute the rights of Bell it may be said that competent opinion seems to accept the originality of the work of Bell.[25]

[22] 'Documents and Dates of Modern Discoveries in the Nervous System,' published anonymously by John Churchill, London, 1834. (This work contains excerpts from the writings of Walker, Bell, Magendie and others and is, I believe, the work of Alexander Walker himself. I have no real proof of this latter statement, however.) In this compilation (p. 28) in the reprint of Walker's 1809 paper (from the *Archives of Universal Science*, II) appears the distinction between the nerves of sensation and the nerves of volition.

[23] Walker, A., 'Sketch of a General Theory of the Intellectual Faculties of Man and Animals,' *Thompson's Annals of Philosophy*, 1815, Art. vi. (Reprinted in 'Document and Dates,' p. 72.) *Cf.*, especially, Eckhard, C., Beiträge zur Geschichte der Experimentalphysiologie des Nervensystems, V, Geschichte der Leitungsverhaltnisse in den Wurzeln der Ruckenmarksnerven,' *Beiträge z. Anat. u. Physiol., von C. Eckhard*, Giessen, 1883, 10, p. 138.

[24] Reprint of 'Idea of a New Anatomy of the Brain,' *op. cit.*, p. 161.

[25] In 1834 Walker published a long work with the following advertisement: 'The Nervous System, Anatomical and Physiological; in which the functions of the various parts of the brain are for the first time assigned; and to which is prefixed some account of the author's earliest discoveries, of which the more recent doctrine of Bell and

Of a very different order from the presumptions of Walker are the claims of the eminent French physiologist M. François Magendie to a share in the discovery of the discreteness of function of the spinal roots of the motor and sensory nerves. This investigator, according to his own statement, arrived independently at the conclusion regarding the difference of function between the two nerve roots. This was accomplished, however, a decade after Bell's discovery had been in print.[26] Magendie insists that when he first performed his experiments he had not heard of Bell's work. He affirms that he learned of the work of the London scientist by reading the 'Manual for Students of Anatomy' which was written by Bell's favorite pupil, John Shaw, in 1821.[27] It must be noted, however, that even this work antedates Magendie's printed accounts of his own experiments. After reading Bell's original paper, Magendie admitted his rival's priority; he continued to urge, nevertheless, that he had advanced the matter in a positive manner over the condition in which it had been left by Bell.[28]

The London scientist himself, seems on the other hand to have accepted Magendie's evidence and to have incorporated parts of it into his own papers without giving due credit to its source. Some critics of Bell have made much of this fact in an unsuccessful effort to deny the validity of Bell's dis-

Magendie, etc., is shewn to be at once a plagiarism, and inversion, and a blunder, associated with useless experiments which they have neither understood nor explained.' He also published an essay in the *Lancet* (1848, pp. 585 *ff.*) upon a similar subject. As noted above, the anonymous 'Documents and Dates of Modern Discoveries in the Nervous System' is but a thinly veiled effort to press the claims of Walker. For the official opinion of the time which is completely favorable to Bell, *cf.* Henry, W. C., 'Report on the Physiology of the Nervous System,' *Report of the Third Meeting of the British Association for the Advancement of Science*, 1833, pp. 62 *ff.*

[26] Magendie's first publications upon this subject are: 'Expériences sur les fonctions des racines des nerfs rachidiens,' *J. de physiol. exper. et pathol.*, 1822, 2, pp. 276 *ff.* and 'Expériences sur les fonctions des racines des nerfs qui naissent de la Moëlle épinière,' *ibid.*, pp. 366 *ff.* These two papers are reprinted in 'Documents and Dates,' *op. cit.* Magendie's own summary of this work appears in the 'Extrait du memoire lu par M. Magendie, à la séance publique de l'Académie le 2 juin, 1823, sur quelques découvertes récentes relatives aux fonctions du système nerveux,' *Ann. de chimie et de physique*, 1823, 23.

[27] Magendie, from the reprint in 'Documents and Dates,' p. 99.

[28] '. . . c'est donc à avoir établi ce fait d'une manière positive que je dois borner mes prétentions.' (Magendie in 'Documents and Dates,' p. 101.)

coveries.[29] While no doubt mistaken in his policy, it is only fair to remark that Bell felt that Magendie had attempted in the first place to rob him of his discoveries.[30]

Much has been written pro and con upon this subject, but the conclusion of the matter seems to be that Bell discovered the fundamental principles of the Law which therefore justly bears his name, but Magendie, perhaps independently, gave the principle an exact formulation and a clear physiological proof. Whether the fact of the structural and functional discreteness of the dorsal and ventral roots of the spinal nerves shall be called 'Bell's Law' or the 'Bell-Magendie Law' is still in dispute.[31] The point in question is trivial. Both scientists did contribute to the knowledge of the principle which sometimes jointly bears their name, but so have many subsequent investigators. Priority seems to be the safest guide, and if first publication be accepted, the usage of 'Bell's Law' must be preferred to any other.

The importance for physiological psychology of the facts embraced in Bell's Law can scarcely be overestimated. Before its promulgation the assumption of the promiscuous conductivity of the nerves made any true understanding of the response mechanisms impossible. After Bell's enunciation, as Sir Charles Sherrington has pointed out, the foundation had been laid for what is called *James' Law of Forward Direction in the Nervous System.*[32] This is a formulation of great importance for physiological psychology.

The demonstration of the discreteness of the motor and sensory nerves is a fact that modern psychology, of whatever

[29] Flint, A., Jr., 'Historical Considerations Concerning the Properties of the Roots of the Spinal Nerves,' *The Quarterly Journal of Psychological Medicine and Medical Jurisprudence*, 1868, 2, pp. 644 *ff*. *Cf.*, also, Waller, A. D., 'The Part Played by Sir Charles Bell in the Discovery of the Functions of Motor and Sensory Nerves,' *Sci. Prog.*, 1911, 4, pp. 78–106.

[30] *Cf.* Bell, C., 'Second Part of the Paper on the Nerves of the Orbit,' *Phil. Trans. Roy. Soc. Lond.*, 1823, p. 307. See also, letter of April 5, 1823 from Charles Bell to George Joseph Bell, 'Letters,' p. 279.

[31] In 1868 Flint listed the authorities up to that time who had used one form or the other, *op. cit.*, pp. 627 *ff*. Since that date this difference in usage still persists.

[32] Sherrington, C. S., 'The Integrative Action of the Nervous System,' 1906, p. 38. For reference to James, W., see: 'Principles of Psychology,' 1890, 2, p. 581. The original publication was ten years before this date. There is no reason to call this 'James' Law' as it was definitely stated by Bell. (*Vide infra* under 'Muscle Sense.')

'school,' accepts. For certain psychologists any knowledge of the nervous system is considered as merely preparatory physiology; for others it is truly a part of psychology. All, however, save a few belated 'mentalists,' admit the importance of a knowledge of the nervous system for one who would understand 'mind.' [33] From the early experiments of Wundt's institute [34] down to the work of the present day there are few laboratory problems which do not make use, in the course of their development, of certain generalizations which are based upon Bell's Law. The principle itself is well established,[35] and its firm foundation has been at the base of much valuable study in the nervous system from the time of Bell to the present day.[36]

Sir Charles Bell, of course, did not write of reflex action in the manner of a physiologist or psychologist of the present day. His was the fundamental work which has made the later erudition possible. Bell was just as willing to write of a nervous action as resulting from 'the will' or from 'an idea' as from a stimulus. He expressly denies, however, that in his researches he sought for the 'seat of the soul.' [37] But

[33] It is interesting to note that the so-called 'introspective psychologists,' who study experience as dependent upon the experiencing individual, and the 'behaviorists,' who study the activity of the organism as a whole, agree at least on the one point of considering physiology as extra-psychological. (*Cf.* Titchener, E. B., 'A Text-Book of Psychology,' 1909, p. 13; and Watson, J. B., 'Psychology from the Standpoint of a Behaviorist,' 1919, p. 19.) Other students admit the study of the nervous system as an integral part of psychology. Thus Professor H. C. Warren holds that self-observation, the observation of behavior, and the observation of the nervous system and its terminal organs are three methods of psychology. (Warren, H. C., 'Elements of Human Psychology,' 1921, pp. 8 *ff.*)

[34] *Cf.* for example, Wundt, W., 'Über psychologische Methoden,' *Phil. Stud.*, 1883, 1, pp. 27 *ff.*

[35] Dr. Howell for example questions Bayliss' conclusions in regard to the so-called 'antidromic impulses' on the basis that if these observations were true they would violate Bell's law. Howell, W. H., 'Text-Book of Physiology,' 1913, p. 81.

[36] Johannes Müller as noted above was a leader in the early development of Bell's law. His researches upon frogs, rather than upon mammals which had been used by Bell and Magendie, firmly established Bell's law in physiological science. *Cf.* Müller, J., 'Bestatigung des Bell'schen Lehrsatzes, dass die doppelten Wurzeln der Ruckenmarksnerven verscheidene Functionen haben, durch neue und entscheidende Experimente,' *Froriep's Notizen aus dem Gebiete der Natur-und Heilkunde*, 1831, 33, pp. 114 *ff.* and a continuation of the same, pp. 128 *ff.*

[37] Bell, Reprint of 'Idea of a New Anatomy of the Brain,' *op. cit.*, p. 153.

he does say, 'The cerebrum I consider as the grand organ by which the mind is united to the body.'[38] It was far from Bell's thought to consider the nervous system, to use Professor Karl Pearson's popular analogy, as an immensely complicated central telephone exchange and as nothing more than such a switchboard.[39] The growth of the mechanistic view of response as including all neural action has been slow.[40] As late as 1896 it was still possible for Professor Dewey to write, with ample justification that "the ordinary conception of the reflex arc theory is a survival of the metaphysical dualism first formulated by Plato . . . ."[41] If 'interaction' and metaphysical entities of all sorts have at last been exorcised from the scientific view of the response mechanism, is it too much to say that this achievement has only been made possible by a patient development of the very principles which were first firmly established by Charles Bell?

III

The Doctrine of the Specific Energy of Nerves

The name of *the doctrine of the specific energy of sensory nerves* was first applied to the theory that each sense organ with its neural connections gives rise, no matter how activated, to a sensation of a particular sort. The name of Johannes Müller is usually associated with the early enunciation of this doctrine; but the facts which form its basis were noticed and recorded at an earlier date by Sir Charles Bell. As Professor Max Dessoir has suggested, the doctrine of the specific energy of sensory nerves might well be termed one of Bell's laws.[42]

[38] *Ibid.*, p. 163.

[39] *Cf.* Pearson, K., 'The Grammar of Science,' 1900, p. 44.

[40] Marshall Hall is usually credited with first having shown that simple reflexes can be explained by reference to the stimulus and the nervous system alone, and without involving any 'conscious sensation' or 'voluntary action.' (*Cf.* Hall, M., 'On the Reflex Functions of the Medulla Oblongata and the Medulla Spinalis,' 1833, pp. 638 *ff.*) It seems, however, that Bell himself had pointed out in public lectures before 1826 that 'an unconscious principle might also be the center of the nervous circle.' ('Sir Charles Bell,' *op. cit.* in *Quar. Rev.*, p. 319.)

[41] Dewey, J., 'The Reflex Arc Concept,' Psychol. Rev., 1896, 3, p. 395.

[42] Dessoir, M., 'Über den Hautsinn,' *Arch. f. Anat. u. Physiol.*, 1892, p. 202.

The following excerpts from the 1811 work of Bell show the early views of the Scotch physiologist upon this subject:[43]

"It is admitted that neither bodies nor the images of bodies enter the brain. It is indeed impossible to believe that color can be conveyed along a nerve; or the vibration in which we suppose sound to consist can be retained in the brain: but we can conceive, and have reason to believe, that an impression is made upon the organs of the outward senses when we see, or hear, or taste. . . .

"It is also very remarkable that an impression made on two different nerves of sense, though with the same instrument, will produce two distinct sensations; and the ideas resulting will only have relation to the organ affected. . . .

"There are four kinds of Papillae on the tongue, but with two of those only we have to do at present. . . . When I take a sharp steel point and touch one of *these* Papillae, I feel the sharpness. The sense of *touch* informs me of the shape of the instrument. When I touch a Papilla of taste I have no sensation similar to the former. I do not know that a point touches the tongue, but I am sensible of a metallic taste. . . .

"In the operation of couching the cataract . . . the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision. . . . But there is an occurrence during this operation on the eye which will direct us to the truth: when the needle pierces the eye, the patient has the sensation of a spark of fire before the eye.

"This fact is corroborated by experiments made on the eye. When the eye-ball is pressed on the side, we perceive various coloured light. Indeed the mere effect of a blow on the head might inform us that sensation depends on the exercise of the organ affected, not on the impression conveyed to the external organ; for by the vibration caused by the blow, the ears ring, and the eye flashes light, while there is neither light nor sound present.

"It may be said, that there is here no proof of the sensation being in the brain more than in the external organ of sense. But when the nerve stump is touched the pain is as if in the amputated extremity. . . .

"If light, pressure, galvanism, or electricity produce vision, we must conclude that the idea in the mind is the result of an action excited in the eye or in the brain, not of anything received,

[43] There are some references to phenomena of specificity in a still earlier work on sensation by Charles Bell. (*Cf.* 'The Anatomy of the Human Body,' 1808, 3, p. 290.)

though caused by an impression from without. The operations of the mind are confined not by the limited nature of things created but by the limited number of our organs of sense. By induction we know that things exist which are not yet brought under the operation of the senses." [44]

In a number of later contributions Bell enlarges upon this view of specificity. He even points out that what would now be called the proprioceptors in the muscles are specific in that they condition sensations of movement, but not of pain.[45] Again, in regard to the eye, he says, "If the retina were sensible to the matter of light only from possessing a finer sensibility than the nerve of touch, it would be a source of torment; whereas it is most beneficially provided that it shall not be sensible to pain, nor be capable of conveying any impression to the mind but those which operate according to its proper function producing light and colour." [46] He adds further that "the nerve of vision is as insensible to touch as the nerve of touch is to light."

In a note appended to the statement just quoted Bell complains that his original pronouncements upon this subject, written twenty years before, had been given too little credit even in England. This fault has never been rectified in full. Occasional writers it is true have given Bell credit for the demonstration of those phenomena which are embraced in the so-called doctrine of specific energy.[47] In general, however, this phase of Bell's work has been passed over. It is difficult to assign a reason for this neglect. Certainly, however, Müller's first work upon specificity did not appear until at least fifteen years after Bell's elaborate consideration of the subject noted above.[48] It was not until 1838 that

[44] Condensed from reprint of the 'Idea of a New Anatomy of the Brain,' *op. cit.*, pp. 155 *ff*.

[45] Bell, C., 'On the Nervous Circle which Connects the Voluntary Muscles with the Brain,' *Phil. Trans. Roy. Soc. Lond.*, 1826, Part II, p. 172.

[46] *Idem*, 'The Hand: Its Mechanism and Vital Endowments as Evincing Design,' London, 1833, p. 152. (This is one of the Bridgewater Treatises.)

[47] *Cf.* Dessoir, *op. cit.*, p. 202.

[48] Rudolph Weinmann, a decided protagonist of Müller, in his 'Die Lehre von den Specifischen Sinnesenergen,' Hamburg, 1895, p. 7, says that Müller's first publication upon *Specific Energy* is his 'Zur Vergleichenden Physiologie des Gesichtssinnes' of 1826.

Müller's view was shaped into final form and published in the *Handbuch der Physiologie*. This book, which was translated almost at once into English, and later into French, won early and deserved popularity. Its pages are, no doubt, the source from which many commentators upon the doctrine derive their information. The work of Bell upon those phenomena which are embraced in the theory of the specific sense energies is not considered by Müller. This lack of recognition is the more surprising because the German experimentalist showed a familiarity with Bell's publications at an early date.[49]

It is by no means improbable that the reason Bell has never received credit for his original conception of specificity is to be found in the fact that he did not give a novel name to the phenomenon which he described. Precedent is not lacking in the history of science to show that one who names a principle, previously described though it may have been, will, superficially at least, be remembered as its discoverer. This failure to name the phenomenon is the purport of the argument set forward by Weinmann [50] in dismissing Dessoir's contention that Bell is to be credited with the origination of the theory.[51] In any case Weinmann's own objection can be given little weight, for he himself admits [52] that he has not read Bell's works. His conclusions are drawn upon the basis of the not wholly accurate quotation of a few of Bell's sentences that is given by Dessoir. Dr. A. Goldscheider, the other historian of the doctrine of the specific energy of the sensory nerves, makes no reference to Bell save in an incidental sentence concerning the muscle sense.[53] These two

[49] *Cf.* Müller, *loc. cit.* in *Froriep's Notzien.* The possibility may also be urged that Müller was indirectly indebted to Bell for certain of the fundamental ideas of 'specific energy.' Alexander Shaw believed (*op. cit.*, p. 226) that Magendie almost certainly was guilty of plagiarism from Bell in announcing in 1824 the specific sensibility of the retina. And yet it is to Magendie that Müller refers in his classical account of specificity ('Elements of Physiology,' 1842, 2, p. 1069).

[50] Weinmann, R., *op. cit.*, p. 20.

[51] Dessoir, *op. cit.*, p. 202. Weinmann's contention is rendered even more doubtful by the fact noted by Dessoir that the name was not itself original with Müller.

[52] Weinmann, R., *loc. cit.*

[53] Goldscheider, A., 'Die Lehre von den Specifischen Energien der Sinnesorgane,' Berlin, 1881, p. 37.

writers, Weinmann and Goldscheider, are the authors to whom many physiologists refer for their history of this doctrine.[54] It is not remarkable, therefore, that most psychologists and philosophers who have made reference to this doctrine have overlooked or underestimated the import of the contribution of Bell. Their information has been derived from sources which in this particular, at least, are not accurate.[55]

It is beyond the scope of the present paper to consider the extension of the doctrine of specific energy to include specificity of quality as well as modality.[56] It should be pointed out, however, that Bell's contribution to the question of specificity is much less open to the charge of being 'the physiological counterpart of a Kantian category'[57] than was the more dogmatic, sweeping, and theoretical formulation of Müller and his followers.[58] As Dr. E. B. Holt and others have pointed out, the Müllerian dogma and its later development into a theoretical basis not only for the mode but the quality of sensations is as much idealistic philosophy as physiology.[59]

As shown above, Bell was interested in presenting what may be called the data of specificity. In presenting these facts it does not seem probable that he was much influenced by

[54] *Cf.* for example, Nagel, W., 'Handbuch der Physiologie des Menschen,' 3, p. 1; and Luciani, L., 'Human Physiology,' 4, p. 55, although in the latter work the reference is given incorrectly.

[55] For example G. H. Lewes neglects Bell in his article, 'L'Hypothèse de L'Enèrgie Spécifique des Nerfs,' *Rev. phil.*, 1876, 1, pp. 161–169. In a later reference to the subject in 'The Physical Basis of Mind,' 1877, 3, p. 207, he adds that he learns from a correspondent that the hypothesis of specific energy was originally propounded by Bell, but he adds 'that it was developed and made an European doctrine by Johannes Müller . . . .' It is strange that Lewes had to be informed of this fact by a correspondent, for in his earlier works in which he makes his surprising argument against the separation of the motor and sensory nerves ('The Physiology of Common Life,' 1860, 2, p. 33 *ff.*) he refers to Bell's work in such a manner that it would seem that he must have seen the references to Specific Energy in that author.

[56] *Cf.* Haycraft, J. B., in Schäfer's 'Text-Book of Physiology,' 1900, 2, pp. 1242 *f.*

[57] Holt, E. B., and others 'New Realism,' 1912, p. 314.

[58] *Cf.* Baly's translation of Müller's 'The Physiology of the Senses, etc.' 1848, from the 'Elements of Physiology,' pp. 1057–1087.

[59] *Cf.* Holt, E. B., *op. cit.*, pp. 314 *ff.* See also, Aubert, H., 'Physiologie der Netzhaut,' 1865, p. 4, and Hauptmann, C., 'Die Metaphysik in der Physiologie,' 1892.

metaphysics.[60] The fundamental fact that he pointed out seems to be that the 'same stimulus' (realistically considered) seems to cause different sensations depending upon the portion of the response mechanism that is activated. Bell pointed out a large number of facts which seem to support this view.[61] It is interesting to notice that these same phenomena lie at the basis of the rather generally accepted distinction between 'adequate' and 'inadequate' stimulation.[62]

Many critical scholars no longer support the notion of the specificity of the sensory nerves at least as originally formulated.[63] The fact remains, however, that right or wrong, this doctrine has exercised an important influence upon physiological psychology. The whole structure of the Helmholtz theories of the senses rests upon an extension of this principle.[64] Indeed, the theory of each mode as it is presented in our current text-books of psychology involves a definite stand upon this question. Bell's pioneer work upon this problem, therefore, still has a fundamental importance for the critical psychologist.

[60] It is difficult to determine how much of Bell's 'physiology was from philosophy.' It seems probable that he knew little of technical philosophy. Possibly he attended lectures by Dougald Stewart (*cf.* Pichot, *op. cit.*, p. 19 and 'Letters' p. 128). There is a reference to John Locke in the 'Anatomy and Philosophy of Expression' (p. 2). In 'The Hand' we find a respectful reference to Dr. Brown 'and philosophers who have been educated to medicine' (p. 195). And in the early work on sensation there are references to Reid ('Anatomy of the Human Body,' 3, p. 136, etc.).

[61] Some of the alleged phenomena of specificity are, however, more dubious than is generally supposed. *Cf.* Nagel, *op. cit.*, p. 7, 'So far as I know we still have no proof that mechanical or electrical stimulation of the optic nerve trunk produces sensations of light' (Translation by E. B. Holt, *op. cit.*, p. 317).

[62] *Cf.* Sherrington, C. S., 'The Integrative Action of the Nervous System,' 1906, pp. 12 *ff.* This relationship was noted many years ago by R. H. Lotze. ('Medicinische Psychologie,' 1852, p. 188.)

[63] *Cf.* Wundt, W., 'Grundzüge der Physiologischen Psychologie,' 1902, 1, pp. 336 *ff.* It is interesting to notice that Bell in 1811 advocated the central specificity to which Wundt refers, in 1902, as novel. For other criticisms see Holt, *op. cit.* and Nagel, *op. cit.*

[64] *Cf.* M'Kendrick, J. G., 'Herman Ludwig Ferdinand von Helmholtz,' 1899, p. 13 and p. 129, also Hering, E., 'Memory: Lectures on the Specific Energies of the Nervous System,' 4th ed. (Open Court translation), pp. 25–41.

IV

The Muscle Sense

Sir Charles Bell was the first definitely to postulate the modern physiological doctrine of the muscle sense and to place this modality on a scientific parity with the five senses of antiquity.[65]

In the early work of Bell there is no direct reference to the muscle sense. In the 'Idea of a New Anatomy of the Brain' of 1811, he says, "But with these nerves of motion which are passing outward there are nerves going inwards; nerves from the surface of the body; nerves of touch; and nerves of peculiar sensibility, having their seat in the body or viscera." [66] This view shows little advance over the older physiological conception of the common sensibility of the body.[67] But for Bell this view, under the guidance of his new principle of the discreteness of the motor and sensory nerves, came to suggest a valid conception of a muscle sense.

At first, apparently, Bell thought that he was the original discoverer of muscular sensibility. In 1833, however, he admits that there were philosophers who had suggested the existence of a sense in the muscles before the idea had occurred to him. He refers specifically in this connection to Dr. Brown.[68] It is true that there are numerous references to the muscle sense in the writings of this philosopher.[69] Sir William Hamilton, however, has shown that even this

[65] *Cf.* Sherrington, C. S., in Schäfer's 'Text-Book of Physiology,' 1900, 2, p. 1006. See also a note in Baldwin's 'Dictionary of Philosophy, etc.,' 1902, 2, p. 121 signed by E. B. Titchener, J. Mark Baldwin, and L. Marillier; and Goldscheider, A., 'Physiologie des Muskelsinnes,' (Gesammelte Abhandlungen, 2), 1898, p. 14.

[66] Bell, reprint of 'Idea of a New Anatomy of the Brain,' *op. cit.*, p. 164. There is a suggestion of this same view in the earlier 'Anatomy of the Human Body,' 3, pp. 138 *f.*

[67] The term, 'common sensibility' remained in use for years after Bell's work. *Cf.* Baly's translation of J. Müller's 'Elements of Physiology,' 1843. It finally outlived its meaning and came to be used in such phrases as 'Das Gemeingefühl der Muskeln' (Weber, E. H., 1846, reprinted in 'Tastsinn und Gemeingefühl, Herausgegeben von E. Hering,' 1905, p. 144).

[68] Bell, C., 'The Hand,' p. 195.

[69] *Cf.* Brown, T., 'Lectures on the Philosophy of the Human Mind,' 1822, 1, pp. 374 *ff.*

Scottish metaphysician was not the originator of the conception.[70] This critic shows that Aristotle hinted at the distinction between the sense of touch and the muscular sense,[71] and that during the sixteenth century it was made comparatively explicit by certain Italian Aristotelians.[72] Descartes also referred to the ideas aroused by the movements of the limbs.[73] Condillac and Maine de Biran made references of a similar sort. The term *Muskelsinn* itself was used before 1790 by certain German philosophers.[74] In none of these writers, and least of all in what Sir William Hamilton aptly terms Brown's 'cloud of words,' [75] is the notion of the muscle sense and its physiological relationship to the rest of the organism clearly given.[76] This was Bell's contribution. After his publication, the *sixth sense*, as it came to be called,[77] was physiologically established.

Bell's most complete exposition of the muscle sense is given in his remarkable paper, 'On the Nervous Circle which Connects the Voluntary Muscles with the Brain' which was read before the Royal Society of London on February 16, 1826. In this communication he shows that 'every muscle has two nerves, of different properties supplied to it.' [78] One of these classes of nerves is connected with motor activities. The other, he asserts, is sensory in function. These sensory muscle nerves are important in the bodily economy for he contends that ". . . we feel the effects of overexertion and

[70] Hamilton, W., 'The Works of Thomas Reid, D.D.,' 1846, pp. 864 *ff.*

[71] For a translation of the significant passage see Titchener, E. B., 'A Text-Book of Psychology,' 1909, note on pp. 161 *f.*

[72] In 1557 by Scaliger; in 1569 by Cæsalpinus, according to Hamilton, *loc. cit.*

[73] Descartes, 'Traité de l'homme,' Chap. 77. Quoted by Henri, V., 'Bibliographie du Sens Musculaire,' *Année psychol.*, 1899, 5, p. 516. *Cf.* also, *ibid*, 'Revue Générale sur le Sens Musculaire,' p. 408. Incidentally it may be noted that this writer acclaims Bell as the founder of the modern view of the muscle sense.

[74] Tittel and Tiedemann are noted among others by Hamilton, *loc. cit.*

[75] *Ibid.*

[76] For a criticism of the older views of the muscle sense *cf.* Mill, J. S., 'An Examination of Sir William Hamilton's Philosophy,' 1865, pp. 270–306.

[77] The term kinæsthetic sense (which now, of course, includes more than merely the muscles sense) was introduced by H. C. Bastian. *Cf.* 'The Brain as Organ of Mind,' 1885, p. 543; See also, Titchener, E. B., *op. cit.*, p. 160.

[78] Bell, C., 'On the Nervous Circle which Connects the Voluntary Muscles with the Brain,' *op. cit.*, p. 164.

weariness, and are excruciated by spasms, and feel the irksomeness of continued position. We possess a power of weighing in the hand:—what is this but estimating the muscular force? We are sensible of the most minute changes of muscular exertion, by which we know the position of the body and limbs, when there is no other means of knowledge open to us. If a rope dancer measures his steps by the eye, yet on the other hand a blind man can balance his body. In standing, walking, and running, every effort of the voluntary power, which gives motion to the body, is directed by a sense of the condition of the muscles, and without this sense we could not regulate their actions." [79]

He next proceeds to show "that a motor nerve is not a conductor towards the brain, and that it cannot perform the office of a sensitive nerve." [80] The evidence which he gives upon this point and which is based upon his own dissections and physiological experiments seems conclusive. The doctrine of the so-called 'sensations of innervation' runs counter to these apparently unequivocal facts, and for this reason is no longer generally accepted. It is remarkable that this false view was given credence long after Bell had presented proofs which were sufficient to refute it.[81]

As is so often the case, in the conclusion of the paper on the 'Sensory Circle' Bell strikes a modern note. "Between the Brain," he says, "and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscles to the brain." [82] Finally, he even gives a clue to what would now be termed the specificity of the proprioceptors in the muscles, for he suggests: "The lower degree of sensibility to pain possessed by the muscles, and their insensibility to heat, is

[79] *Ibid.*, p. 167.

[80] *Ibid.*, p. 168.

[81] Sherrington shows that the view which dispenses with peripheral organs and afferent nerves for the muscular sense has had powerful adherents (*op. cit.* in Schäfer's 'Text-Book of Physiology,' pp. 1002 *ff.*). Among those who held this view in some form are mentioned Bain, Helmholtz, Lewes, Mach and Wundt. Bell at one time, he tells us, considered the sensations of innervation view ('The Hand,' p. 199) but he abandoned it after his experiments were completed.

[82] Bell, C., 'On the Nervous Circle, etc.,' *op. cit.*, p. 170.

no argument against their having nerves which are alive to the most minute changes of action in their fibers." [83]

Bell points out the importance of the muscle sense in the empirical development of the other modes. "The sensibility," he says, "of the skin is the most dependent of all on the exercise of another quality. Without a sense of muscular action or a consciousness of the degree of effort made, the proper sense of touch could hardly be an inlet of knowledge at all." [84]

In showing the relationship between vision and muscular sensations Bell cites the following case: "A mother while nursing her infant was seized with a paralysis, attended by the loss of power on one side of her body and the loss of sensibility on the other side. The surprising, and, indeed, the alarming circumstance here was that she could hold her child to her bosom with the arm which possessed muscular power, but only so long as she looked to the infant. If surrounding objects withdrew her attention from the state of her arm, the flexor muscles gradually relaxed and the child was in danger of falling." [85] This case shows among other things, Bell remarks, "how ineffectual to the exercise of the limbs is the continuance of the muscular power, without the sensibility which should accompany and direct it." [86] In this statement there is a suggestion of the modern conception of the muscular activity in which the proprioceptive apparatus is shown to be fundamental for tonus and the muscular mechanics in general.[87]

The importance of the motor apparatus in perception is further emphasized by the statement that the "property of the hand in ascertaining the distance, the size, the weight, the form, the hardness or softness, the roughness or smoothness of objects results from there being this combined per-

[83] *Ibid.*, p. 172.

[84] Bell, C., 'The Hand,' pp. 194 *f.*

[85] *Ibid.*, p. 204. This is 'Charles Bell's well-known old case of anæsthesia' to which Professor James refers ('Principles of Psychology,' 2, p. 492) without, however, giving any reference to Bell's written works.

[86] *Idem*, 'The Hand,' p. 204.

[87] *Cf.* Sherrington, C. S., 'Some Aspects of Animal Mechanism,' *Science*, N.S., 56, 1922, p. 346.

ception—from the sensibility of the proper organ of touch being combined with the consciousness of the motion of the arm, hand and fingers." [88] He further remarks upon the pleasures which he thinks are conditioned by the muscular sense, such as athletic exercise, dancing, and, to some extent, music. In the latter connection he urges with telling argument the view that rhythm is to a considerable extent dependent upon the muscular sense. He adds that, "There is thus established the closest connection between the enjoyments of the sense of hearing and the exercise of the muscular sense." [89]

In writing on the physiology of the eye, Bell notes that too much emphasis is given to the ball of the eye and the optic nerve. The muscles of the eye, he urges, are also important for visual perception. "It is to the muscular apparatus," he says, "and to the conclusions we are enabled to draw from the consciousness of muscular efforts, that we owe that geometrical sense by which we become acquainted with the form, and magnitude, and distance of objects." [90]

Quotations could be multiplied to show that Bell considered as most important the motor side of the sensory-neuro-muscular mechanism. He was far, of course, from enunciating a complete 'motor theory of consciousness,' and yet to the modern reader there is much in his writings to suggest this view. In his paper on the 'Sensory Circle' he opened the way for a consideration of the so-called circular and chain reflexes which many modern psychologists consider as fundamental to an understanding of behavior.[91]

There exists an unfortunately incomplete summary by

[88] Bell, C., 'The Hand,' p. 204. To see how little fundamental change there has been in the consideration of these blends in the years since Bell's writing *cf.* Titchener, E. B., 'A Text-Book of Psychology,' 1909, pp. 171 *f.*

[89] *Ibid.*, p. 207. For a comparison between these old considerations and a modern view, *cf.* Sanford, E. G., 'The Function of the Several Senses in Mental Life,' *Amer. J. Psychol.*, 1912, 23, pp. 59–74.

[90] *Idem*, 'On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit,' *Phil. Trans. Roy. Soc. Lond.*, 1823, part I, p. 167. *Idem.*, 'On the Fourth and Sixth Nerves of the Brain,' *Trans. Roy. Soc. Edinburgh*, 1838, 14, pp. 237 *ff.*

[91] *Cf. e.g.*, Allport, F. H., 'Social Psychology,' 1924, pp. 39 *ff.*; and Baldwin, J. M., 'Mental Development in the Child and the Race,' 1895 (2d Ed.), pp. 217 *ff.*

another hand of a paper read by Bell before the Royal Society of Edinburgh 'On the Necessity of the Sense of Muscular Action to the Full Exercise of the Organs of the Senses.' [92] In this paper he pointed out that something more is needed than the mere activation of the sensory nerve in order to give perception. This additional factor, he shows, is muscular movement. In this connection he refers to the then novel experiments of Weber and others on what we now term the two-point limen. In criticism of this work he says, in the words of his reporter, "Those experimenters drew the conclusion that this capacity of distinguishing the two points resulted from the greater concourse of nervous filaments. He stated, on the contrary, that the capacity of distinguishing the two points, or in fact of distinguishing the form of bodies did not result from the mere sensibility of the part. . . . He contended, on the contrary, that the capacity resulted from the junction of sensibility and motion." [93] In this paper also he points out that "weight so little different as a sovereign and a shilling could be distinguished when placed on the tip of the finger," but in no other place. In general he affirms that the sensibility of the difference in weight or of the perception of the two points will be greater, the more motile the part stimulated. He considers particularly the tongue, fingers, and lips. On the basis of certain of his operations he also points out that he believes the visual perception of objects in space to depend upon the eye muscles.[94]

Bell's work, therefore, upon the physiological nature of the muscle sense not only established it as the 'sixth modality'; but it went further still, and suggested many of the factors which introspective and behavioristic experimentation has later attributed to the proprioceptive system.

[92] Read Feb. 22, 1842, *Proc. Roy. Soc. Edinburgh*, 1, pp. 361–363.
[93] *Ibid.*, p. 362.
[94] *Ibid.*, p. 363.

V

Bell's Views on the Five Senses

In 1808 John and Charles Bell published a work upon 'The Anatomy of the Human Body,' [95] in four volumes. Of the two volumes which Charles wrote, the first was devoted to a consideration of the structure and the physiology of the nervous system and the organs of sense. This work shows a knowledge derived from actual anatomy and a familiarity with the literature of the subject as it existed at that time. It is not, however, marked by that originality which is so notable a characteristic of much of Bell's later work. It is rather a compendium of accepted facts than the adventure of an experimentalist beyond the frontier of the known. However even in this work Bell had begun what he later termed the 'correction of books from the great book of Nature.' [96] In one place in this early work he exclaims, "How superior is simple experiment to the most ingenious speculation"! [97]

In the consideration of almost every topic of sensation he reviews the work of previous investigators, adding where he can his own observations. He makes note of the fact if he describes a phenomenon which he has not himself observed. Probably twenty years after this original publication on the senses Bell again wrote upon the subject. This work is an undated popular monograph entitled 'The Organs of the Senses Familiarly Described, Being an Account of the Conformation and Functions of the Eye, Ear, Nose, Tongue, and Skin; Illustrated by Twenty Coloured Plates." [98]

In his treatment of vision he passes from a consideration of physical optics to a minute discussion of the structure of the eye. He attempts to simplify terminology where possible. He then turns to consider a number of specialized topics of the psychophysiology of vision. He cites in detail an account,

[95] The paging given in this paper is that of the American reprint of this work, New York, 1809.

[96] Bell, C., 'Letters,' p. 159.

[97] *Idem*, 'The Anatomy of the Human Body,' 3, p. 183.

[98] This small book is in few ways an advance over the earlier work and it will therefore be given little consideration in this paper.

for example, of post-rotational nystagmus, as it would now be termed, which was studied by means of the projection of the after image of a candle upon a white surface.[99] A chapter is devoted to a consideration 'Of the Manner in Which the Eye Adapts itself to the Distance of Objects.' Here the varying theories of the time are weighed and one and all rejected. The conclusion, however, while suggesting the opinions of certain better known psychologists, is not important, for it depends upon the invocation of 'attention' rather than the suggestion of a mechanical explanation.[100] The significance of the stimulation of corresponding points in each retina and the parallel motion of the eyes is considered.[101]

The physical basis of sound is first considered in the chapters on the ear. The comparative anatomy of the auditory organ is then presented and hearing in crabs, lobsters, fish, reptiles, amphibians, birds, and lastly man, is treated. The morphology of the external and middle ear is described in characteristic fashion. Bell taught that there are four ear bones, the one additional to the three now usually considered being the small *os orbiculare* between the *incus* and *stapes*.[102] The treatment of the inner ear is, of course, inadequate as judged by modern standards; and yet here again Bell seems to have displayed admirable perspicacity in prediction. As recent students have said in reference to Bell, "It is remarkable that some of the older anatomists conjectured, long before the discovery of the organ of Corti, that this part of the internal ear was in some way analogous to a musical instrument having wires of various lengths tuned to tones of various pitches." [103]

[99] Bell, C., *op. cit.*, p. 184.

[100] *Ibid.*, p. 216.

[101] *Ibid.*, pp. 218 *ff.* *Cf.* also, *idem*, 'On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit,' *Phil. Trans. Roy. Soc. Lond.*, 1823, Part 1, pp. 166–187.

[102] *Idem*, 'The Anatomy of the Human Body,' 3, pp. 253 *ff.* *Cf.* also, *idem*, 'The Senses, etc.,' p. 61.

[103] M'Kendrick, J. G., and Gray A., in Schäfer's 'Text-Book of Physiology,' p. 1168. The reference given to Bell is Vol. 3 of the 'Anatomy of the Human Body' but to the 1816 Edition, which the present writer has not been able to inspect. In the first edition of this work the analogy is not quite so clearly suggested. *Cf. op. cit.*, p. 276. See also, Wilkinson, G., and Gray, A., 'The Mechanism of the Cochlea,' 1924, p. 137.

Bell's consideration of smell seems quite inadequate to modern eyes. He points out the supposed utility of the sense in protecting the individual from bad air; but in regard to olfactory physiology he is vague. "Smelling," he says, "seems to be the least perfect of the senses. It conveys to us the simplest ideas, and is least subservient to the others. The sensations it presents to us we can less easily recall to memory; and the associations connected with it are less precise and definite than those of the senses of hearing and seeing." [104]

In considering gustation Bell points out that the papillæ are the organs of taste, and that, "if we take a pencil and a little vinegar, and touch or even rub it strongly on the tongue, where those papillæ are not, the sensation only of cold liquid is felt . . . ." [105] Moreover, as is his custom he not only refers to the sensation of taste but he emphasizes as well the respônse side of gustatory stimulation. "A curious circumstance, in the sense of taste," he writes, "is its subservience to the act of swallowing. When a morsel is in the mouth and the taste is perfect, our enjoyment is not full: there follows such an excitement of the uvula and fauces, that we are irresistibly led to allow the morsel to fall backward, when the tongue and muscles of the fauces seize upon it with voracious and convulsive grasp and convey it to the stomach." [106]

Lastly in the consideration of touch he points out that the pressure qualities are distinct from heat and cold. In regard to these last two he remarks that, "Now, although heat be a quality, and cold the privation of that quality, yet in relation to the body, heat and cold are distinct sensations." [107]

VI

The Reciprocal Innervation of Antagonistic Muscles

The phenomenon of reciprocal innervation may seem rather the concern of the physiologist, as such, than of the

[104] Bell, C., 'The Anatomy of the Human Body,' 3, p. 280.

[105] *Ibid.*, p. 290. This is the first place in the writings of Bell in which a fact of specificity of a sense organ is recorded.

[106] *Ibid.*, p. 290.

[107] *Ibid.*, p. 292.

physiological psychologist. There is, however, a growing tendency on the part of certain psychologists to attach importance to this phenomenon in an understanding of mental life.[108]

The history of the knowledge of this fact of response is an interesting one. Sir W. M. Bayliss has shown that, once again, with remarkable prescience, Descartes speculated upon reciprocal innervation.[109] It remained, however, for Sir Charles Bell to present not only a theoretical description of this phenomenon, but an experimental demonstration as well. In a paper read before the Royal Society of London, Bell observes that, "The nerves have been considered so generally as instruments for stimulating the muscles, without thought of their acting in the opposite capacity, that some additional illustration may be necessary here. Through the nerves is established the connection between the muscles, not only that connection by which muscles combine to one effort, but also that relation between the classes of muscles by which the one relaxes while the other contracts. I appended a weight to a tendon of an extensor muscle, which greatly stretched it and drew out the muscle; and I found that the contraction of the opponent flexor was attended with a descent of the weight, which indicated the relation of the extensor. To establish this connection between two classes of muscles whether they be grouped near together as in the limbs or scattered widely as in the muscles of respiration there must be particular and appropriate nerves to form this double bond, to cause them to conspire in relaxation as well as to combine in contraction." [110]

In commenting on this principle as set forward by Bell, Sir Charles Sherrington gives an appreciation of its value; but he suggests that Bell pictured a reciprocal innervation

[108] *Cf. e.g.*, Gates, A. I., 'Psychology for Students of Education,' 1924, pp. 61 *f.*; Weiss, A. P., 'A Theoretical Basis of Human Behavior,' 1925, p. 357; Givler, R. C., 'The Ethics of Hercules,' 1924, p. 32; Holt, E. B., 'The Freudian Wish and Its Place in Ethics,' 1915, p. 75, and others.

[109] Bayliss, W. M., 'Principles of General Physiology' (4th Ed.), 1924, pp. 494 *ff*.

[110] Bell, C., Second part of the paper on the 'Nerves of the Orbit,' *Phil. Trans. Roy. Soc. Lond.*, 1823, pp. 295 *f.* (Footnote.)

that originated at the periphery. Recent research has shown that this function is probably centrally determined.[111]

While possibly not so important as certain other contributions, Bell's work on reciprocal innervation must still be held to have a true significance for those who would present a complete consideration of physiological psychology.

VII

The Expression of the Emotions

When Charles Bell set out upon his memorable journey to London to make adventure of his fortunes in the metropolis he carried with him the manuscript of a work upon anatomy in its relation to the fine arts.[112] After some effort and with the sanction of Benjamin West, the Pennsylvania-born President of the Royal Academy, he published this work in 1806 as 'An Essay on the Anatomy of Expression in Painting.' This volume won wide recognition. It was even read by the Queen.[113] The book went through several editions and was not in definitive form until the third edition of 1844. It was then published under the altered title of 'The Anatomy and Philosophy of Expression as Connected with the Fine Arts.'[114]

The success of this work was not an accident. Bell was singularly fitted to write upon this subject. He was at once a skillful anatomist, a thoughtful physiologist, a student of the history of the fine arts, and a painter of no mean ability. This combination of interests and talents early led him to the study of the face and its expressions. The result of this study was a notable contribution to knowledge. Charles Darwin, in the introduction to his own work upon expression, pays the following tribute to Sir Charles Bell: "He may with justice be said not only to have laid the foundation of the

[111] Sherrington, C. S., 'The Integrative Action of the Nervous System,' 1906, p. 287. *Cf.* also, Lucas, K., and Adrian, E. D., 'The Conduction of the Nervous Impulse,' 1917, pp. 93-102.

[112] Pichot, A., *op. cit.*, p. 42.

[113] Moore, N., 'Sir Charles Bell,' *Dictionary of National Biography*, 1885, 4, p. 154. *Cf.* also, 'Letters,' p. 32.

[114] References in this paper are to the Sixth Edition of 1872, which is based on the Third Edition.

subject as a branch of science, but to have built up a noble structure. His work is in every way deeply interesting; it includes graphic descriptions of the various emotions, and is admirably illustrated." [115]

As in the case of other parts of the original work of Bell there have been rival claimants for the honor of having founded the scientific study of expression. There seems to be no doubt, however, that the work of Bell preceded that of M. Moreau, his main opponent.[116]

In his work on the expression Bell first considers the relationship between the form of the bones of the head and the permanent contours of the face.[117] The neuromuscular mechanism of transient expression, however, is given the greatest attention. To the facial muscles Bell attributes a large share of the totality of expression which makes up the general physical picture of an emotion.[118] He believes that the facial muscle nerves are intimately connected with the respiration center in the central nervous system.[119]

Bell's whole view of the emotions was commendably far from mere intellectualism. It would not be hard to show by isolated quotation, indeed, that Bell had some inklings of that theory of the emotions which was, with different shades of meaning, promulgated by William James and Karl Lange.[120] Bell writes, for example, that:

"If, in the examination of the sources of expression, it should be found that the mind is dependent upon the frame of the body, the discovery ought not to be considered as humiliating. . . .

"In examining the phenomena of mind, philosophers have too much overlooked this relation between the mental operations and the condition of the bodily frame. It appears to me that the frame of the body, exclusive of the special organs of seeing, hearing, &c. is a complex organ, I shall not say of sense, but which ministers

[115] Darwin, C., 'Expression of the Emotions in Man and Animals,' 1873, p. 2.

[116] This assertion is based upon Darwin's study of the matter. *Cf. op. cit.*, p. 3. Bell, of course, was not the first to write upon the general facts of expression.

[117] Bell, C., 'The Anatomy and Philosophy of Expression,' pp. 19–53.

[118] *Ibid.*, pp. 97–144.

[119] *Cf.* James, W., 'The Principles of Psychology,' 2, p. 483. (Footnote).

[120] *Cf.* 'The Emotions' (by William James and Karl Lange), edited by Knight Dunlap, 1922.

like the external senses, to the mind; that is to say, as the organs of the five senses serve to furnish ideas of matter, the framework of the body contributes in certain conditions to develop various states of the mind." [121]

The detailed application of this interesting, and, for its time, bold speculation in regard to the emotions leads Bell into a consideration of the muscle groups which characterize the various expressions. This work is still valuable, for it is based upon an extensive study by a skilled anatomist, not only of the human form as seen in classical and modern art, but also upon a detailed observation of the portrayal of emotion by actors.

In general Bell's views upon expression are descriptive. He was of course not motivated by a desire to show the phylogenetic evolution of expression. For that reason the present-day psychologist who rejects the naïve nativistic view that expressions are merely the inherited vestiges of serviceable habits acquired in racial history may find less to 'explain away' in Bell's work than in that of Darwin.

It may therefore be said with assurance, that in his important work of the physical basis of expression, Bell contributed largely to a subject which still rightly attracts the interest and study of experimental psychology.[122]

Summary and Conclusions

In this paper an effort has been made to show that Sir Charles Bell was a pioneer in the scientific study of many subjects which are now treated as important in a systematic consideration of physiological psychology. To the fundamental conception of the response mechanism he contributed the basic distinction between the functions of the afferent and the efferent nerves. Grounding his argument upon this discovery, he developed a notion of what was later termed

[121] Bell, C., 'The Anatomy and Philosophy of Expression,' pp. 82 *f.*

[122] *Cf. e.g.*, Langfeld, H. S., 'The Judgment of Emotions from Facial Expression,' *J. Abn. Psychol.*, 1918–1919, **13**, pp. 172–184; Feleky, A. M., 'The Expression of the Emotions,' Psychol. Rev., 1914, **21**, pp. 33–41; Ruckmick, C. A., 'A Preliminary Study of the Emotions,' Psychol. Monog., 1922, **30**, pp. 30–35; Boring, E. G., and Titchener, E. B., 'A Model for the Demonstration of Facial Expression,' *Amer. J. Psychol.*, 1923, **34**, pp. 471–485.

the specific energy of nerves, a doctrine which still challenges psychologists. From his experiments and publications developed the modern view of the muscle sense; he contributed also to the knowledge of the other modalities. The reciprocal innervation of antagonistic muscles received from him an early physiological demonstration. His illuminating consideration of the expression of the emotions, unbiased by any Darwinian assumptions are in many ways valid even today.

Such were the contributions of this Scottish surgeon who interested himself from the love of science in certain of those physiological processes which modern psychology considers fundamental to a knowledge of mind. Much of his work was crude; but he was a pioneer. His knowledge of technical psychology was slight; but his research has made possible many notable advances in the knowledge of mental life. On the roll of the founders of physiological psychology the name of Sir Charles Bell must have honorable inscription.

LOCAL SIGNS AS ORIENTATION TENDENCIES[1]

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If a subject is stimulated equally with a sharp point now on the hand and now on the forehead, say, while blindfolded, and is asked to say what took place, he will probably describe the painful stimuli as being alike but differently located, and he will be able with a high degree of accuracy to say where each stimulus was applied. If he is not asked for verbal responses he will withdraw in each case the part of the body that is stimulated. Experiments have shown that the localization of such responses, however given, is far more accurate in certain cutaneous areas, as on the finger tips, than in others, as on the back. Corresponding to these different degrees of accuracy in locating the stimulus in different areas are also different degrees of discrimination of two-point stimuli; that is to say, in certain localities two points can be discriminated, or reacted to, as two instead of one, when simultaneously applied, at much smaller distances of separation than is possible in other localities. The distance of separation of the points necessary for discrimination at any given locality is considerably less for successive than for simultaneous stimuli. In these experiments it is, of course, now and then necessary to touch with only one point to test the subject's consistency. It is known, moreover, that this discriminability in any area increases with practice and that it decreases rather rapidly again with cessation of practice (Volkmann); but the extent and nature of transfer of such practice needs further investigation. Discriminability is also affected, though less regularly, by such factors as fatigue, drugs, and various sets or attitudes. Indeed, so important are some of these factors that one investigator has concluded

[1] Read at the Cornell meeting of the American Psychological Association, December 28, 1925.

that the discrimination threshold of any subject is so variable as to make its scientific determination impossible.[2] It was early found that the fineness of two-point discrimination on any area of the skin is a function of mobility (Vierordt). It is doubtless wrong to say that mobility and discriminability have a one-to-one relationship, since such conditions as the possibility of bringing the area in question into the range of the visual field, the ease of touching the area with other movable parts like the hands, the thickness of the skin, etc., also complicate the relationship. The correspondence seems, however, to be rather close.

So much for the more general, objectively verified facts, and we must recognize that there are many more or less important details still in dispute. As to explanations of even the more generally accepted facts, important differences remain to be settled. It is well known that certain subjective accounts have attributed the ability to localize and to discriminate tactual stimuli to 'local signs,' these being some sort of 'feeling' or perception of location.[3] Such accounts come dangerously near the error of assuming what is to be explained. The extreme nativists have avoided the difficulty of accounting for local signs, by the assumption that each sensation carries with its unique sensory quality, by original nature, also certain localizing qualities, but they have not successfully analyzed these 'qualities,' nor have they shown how they operate. Moreover, this assumption, while it really does not explain anything, carries a certain objectionable import as to heredity. The empiricists, on the other hand, have held that local signature is acquired and to an extent at least analyzable; that it is a combination of sensory *qualia* with motor sensations derived from actual movements. Berkeley, as is well known, applied this second view to vision, assuming, however, too much explicit awareness of sensations—visual, tactual, kinesthetic—which according to his theory became

[2] Binet, A., *Année psychol.*, 1903, **9**, 247–252.

[3] J. Mark Baldwin, in his *Dictionary of Philosophy and Psychology*, 1902, defines local sign as "A specific 'shading' or 'coloring' of certain sensations whereby every such sensation is invariably referred to a definite position upon the organ or elsewhere is space."

associated through movement. Various modern theories have roughly followed Berkeley's approach. Von Thunberg says, "For the explanation of our ability to locate a cutaneous sensation at the point of the stimulus, one must assume that with stimulation of the elements of the corresponding nerve centers in the brain, certain factors operate which make different the psychical effect of stimulation of one element from that of another. Every element has, therefore, its own individual or special sign, which is used for the localization of cutaneous sensations within the spatial perception-picture of the surface of the body. This individual sign of the element in the experiencing center must, according to the law of outward projection, be recognized by consciousness as a characteristic of the nerve ends." [4]

Here surely we have an 'explanation' which for subjectivity and complexity cannot be excelled even by the ancient astrologers,—and it explains nothing at all! How do the nerve ends become localized by this mysterious conscious entity so completely shut up in the brain? Let us try to get a simple and general conception of the problem that is before one who holds this view of local signs. An illustration may serve best to put the situation clearly before us. If you call the telephone 'central' in your city—assuming that the automatic switch system has not been installed—she may immediately think of you as in a certain location, or react to you as being in a certain part of the city, in a certain building, and so on,—*if* she knows your voice and your home well enough, and *if* in addition she is aware that you are calling from your home. If 'central' does not know your voice and your place of residence it might be possible to locate you by testing and following up the several wires connected with the receiver of the telephone in her office. This would be a complicated process and would illustrate one possible, though incredible, type of theory of location of touch on the body; it does not illustrate any empirical view that has actually been developed.

[4] Translated from Nagel's 'Handbuch der Physiologie des Menschen,' 1905, 3, 712–713.

Both of these means of locating the person calling 'central' suggest, when applied to our problem, an inner experiencing agent concealed somewhere in the brain and shut out from the world of our activities, a view that is now impossible to any psychologist. Our illustration is, however, not so inappropriate for bringing out the essential features and weaknesses of older dualistic theories. For them the 'soul' had no way of getting information of the 'outer world' except through the 'avenues of the senses,' and their influence on much current thought in psychology is still obvious. Thus in the quotation given, every point on the body surface must have 'in the experiencing center,' where consciousness is assumed to sit alert to the signaling, its own characteristic sign, which must be of such a nature that it can attach itself to any kind of sensation aroused at this location. For Wundt these signs are really qualitatively different sensations each representing some location. He often speaks from the introspective point of view of these different localizing signs as the 'feeling' of the sensations at different positions. In the visual field Wundt points out such local sign differences in color as the decrease in saturation and change in hue toward the periphery. These different sensations, then, become associated or creatively synthesized with sensations of the movement excited by them in such a manner that the position of each stimulus is immediately given 'in consciousness.' Local signs are thus, in this view, slight modifications of sensation which serve to indicate just where the skin is stimulated, and, in Warren's words, "are due not to the stimulus but to the receptor, and are similar for all sensations from a given receptor." [5]

Lotze, who originated the theory of local signs, was less inclined in his first statement than later to make local signs 'sensations,' regarding them rather as characteristic nervous processes varying with the locality stimulated,[6] but as the psychology of sensations was then dominant these processes later became 'sensations of movement,' or 'motor sensations.'

[5] Warren, H. C., 'Elements of Human Psychology,' 1922, 406.
[6] See McDougall, W., 'Outline of Psychology,' 1923, 241.

James speaks of the relations between the local sign and the thing signified, rather than of sensations of movement, and clearly says, "if a sign is to lead us to *the thing* it means, we must have some other source of knowledge of that thing." [7] He insists that either the thing has been given in a previous experience of which the sign also formed a part—they are associated—or it is given by some sort of innate cognition. The latter alternative he rejects, for the machinery by which the sign and the thing signified become associated 'is already familiar to us. It is neither more nor less than the law of habit in the nervous system.' Unfortunately he does not meet the difficulties—they were not recognized in his day—that arise with the association laws in such habit formation, though he was clearly on the right track. His general position is so well known as not to need further attention here. Stout emphasizes the differences in the quality of sensations according to the point of the tactile surface stimulated, but he totally rejects association as the basis of these differences "because unless the qualities of contact *n* and contact *t* were different, association with *n* would be the same as association with *t*. *T* would tend to reproduce whatever *n* tended to reproduce, and there would be no distinction between them." [8] Thus he goes back to the barren logic of non-spatial conscious entities. For Stout active movement, based on a process having unity and continuity of interest, builds up 'cumulative dispositions' in which local signs come to have spatial reference or to prompt the movements for which they stand (p. 340). In more recent years Stout, Lloyd Morgan, William McDougall, and others have practically adopted a nativistic theory as the basis of spatial perception, a position to which the observed accuracy and apparent anticipation of results of movement in certain native reactions of animals like the chick have driven them.[9] Until we regard the facts of learning as adaptation of unitary living organisms and give up dualism, either this view or some empirical view of sensory

[7] James, W., 'Principles of Psychology,' 2, 158.

[8] Stout, G. F., 'A Manual of Psychology,' 1899, 332.

[9] See the symposium on 'Instinct and Intelligence,' *Brit. J. Psychol.*, 1909–10, 3, 209–270, and also McDougall, W., 'Outline of Psychology,' 235–246.

signs as indicators to an 'inner consciousness' of external spatial relations, is all that psychology offers as a solution of how we locate stimuli.

"In view of all the evidence," say Ladd and Woodworth after a review of known facts (as determined from the dualistic view), "it would seem that the general theory of local signs must be constructed in somewhat the following way: Within certain limits, which it is impossible for science yet definitely to fix, the irritation of the different nervous elements of certain organs of sense gives rise to sensations which differ in the *shading* of their quality according to the locality in the organ at which the elements are situated. This is probably true of both peripheral and central areas of the total organ. It is true of the latter areas as dependent on the excitation of the former. The simultaneous irritation of several locally related elements of the organ (and the irritation is seldom or never confined to a single element) results, then, in a certain *mixture of feeling* dependent upon the number and local relation of all the elements thus simultaneously irritated," and "every sense which is the medium of space-perceptions has a system of local signs of its own." [10] This statement is a good illustration of the older dualistic treatment of local signs with its hopeless entanglements, and is not revised so as to be related closely to the newer developments in learning so well summarized in a later chapter by these authors.

Angell seems to have been about the first writer to relate local signature to the trial-and-error learning process, and thus on this problem to use the better tool for simplifying explanations of behavior, for which we are indebted to Lloyd Morgan. In his 'Psychology,' first published in 1904, he explicitly recognized that the establishment of one's orientations to stimuli on different parts of the body comes about through some sort of trial-and-error procedure. He says that in learning his space-world the child does not use the local signs in a "reflective way, but generally employs the 'try,

[10] Ladd, G. T., and Woodworth, R. S., 'Elements of Physiological Psychology,' 1911, 389 *f*.

try again method,' until he hits the mark. But his success carries with it a recollection of the total feeling of the successful experience, and in this total feeling the local element is an indispensable part, even though the child is not himself definitely cognizant of the fact" (p. 148). But even in this statement the 'total feeling' seems to be the locating element, the method by which this indication of position is accomplished being still in the dark. We are still not free from a hampering dualism.

Carr, in his new book, has gone further. Defining local signs as the "differential aspects of the *stimulus* [11] that correspond to the various locations of the object," he holds that "the ability to adapt successfully to the varying position of an object thus depends upon the development of a system of associations between each local sign [*i.e.*, for him, between each stimulus difference] and an appropriate localizing response. In the process of mental development," he continues, "a person first associates an overt localizing response with each local sign. As these associations become firmly established with use, the location of an object . . . can be appreciated as a meaning without the necessity of responding in an overt manner. The subject expresses these spatial meanings of objects in terms of verbal judgments, but these judgments of locality, like the overt localizing responses from which they are genetically derived, are also based upon this system of local signs." [12]

Carr does not show how specific orientations are acquired, and his use of 'local sign' as a differential aspect of the *stimulus*, though easily applicable to vision and hearing involving bilateral receptive organs, has difficulties when applied to single cutaneous stimuli. In such a case the stimulus has differential aspects only as an element of a larger situation involving "the previous activities of the organism." It thus becomes unsatisfactory, if we are to deal with specific facts, to treat local signs as qualities of the stimuli rather than as specific orientation habits and tendencies.

[11] Italics mine.

[12] Carr, H. A., 'Psychology,' 1925, 142 *f*.

These tendencies and habits are of course not separable from the situations in which they are developed and defined. Indeed, it would conduce to clearness, and to the avoidance of misunderstanding through association with old terms, if the term 'local sign' were entirely dropped from psychology; and since there is nothing to distinguish local signs from other forms of specific habits, the discontinuance of the term is here recommended. The alleged specific qualities of sensations which give them location, and which have usurped so much attention in psychology, really explain nothing at all, whether or not they exist; moreover, the postulation of such local signs has, because of its dualistic implications, given to psychology only entanglements, difficulties, and controversies. The problem of how orientation tendencies to local stimuli arise is a real one, however, and Carr's objective handling of the data connected with it is a step in advance. But he does not show specifically just how these tendencies are set up. With subjective terms cleared away, which really only cover up the need of an explanation, let us inquire how these specific orientations to cutaneous stimuli become established.

All reactions depend on some sort of innate organization. In the higher organisms many reflex adjustments and protective acts are definitely determined by innate mechanisms, e.g., the eye-wink, sneezing, coughing, etc. Practice may specialize more definitely many of these responses and build up many additional ones. In man so many reaction patterns become established that one easily falls into illusions and hallucinations (wrong reactions) by slight and unusual changes in the total stimulation complex. Numerous illustrations will occur to any reader. There is plenty of room here to organize on an objective basis all the experimental data on perception, which behaviorists have eschewed because of their dualistic implications. Attempts at neural explanations of the origin of specific orientations have usually ended with some statement of association, but without indicating just what in such cases becomes associated. Even if sensations do become associated, does that explain the actions involved? Certainly not, without further assumptions. The controversies between

the nativists and the empiricists have shown the futility of such 'explanations.' The method of approach here proposed makes all such controversies needless and irrelevant.

Suppose 1, 2, 3, 4, and 5 represent the different more or less individually mobile arms of a starfish, and that, say, 3 is strongly stimulated by some sharp object. If there is no innate difference in the several neural tracts leading from the affected receptors into the motor channels of this, or any other arm, any response is about as likely to occur as any other. Let us suppose that the arm 4 responds,—imperfectly, of course. There will be no change in the stimulation, except that its effects will presumably increase cumulatively with time. Next the motor impulses may effect a response of arm 1, or of 1, 4, and 5 together or in any random order and with different intensities. Still no important change occurs in the sensory impulses from the stimulus. Only when 3 responds, either alone or in any combination with the other arms, will the incoming impulses from the stimulus be changed. If its reaction is toward the stimulus the impulses will presumably be increased at the same moment, while if the reaction is away from the stimulus they will cease; with any other movement of this arm there will be concurrent changes in the exteroceptive impulses from the stimulus. Since the movement of any other arm than the one stimulated is only incidentally and not necessarily connected with a sudden variation in the stimulus, and since any appreciable movement of 3 is invariably and necessarily so connected, it is to be expected on known physiological principles that all movements except those of 3 will eventually become weaker than movements of 3, and finally drop off entirely as irrelevant. But since sensory impulses tend to become directed into the motor channels that are active when they occur, very definite associations between changes in the stream of sensory impulses from the stimulus and the motor impulses which effect the movement of 3 will eventually become established in the form of a specific habit pattern. Proprioceptive impulses involved in the relevant movements will likewise, and on the same principle, become organized into the same habit complex.

Thus an orientation response to a stimulus anywhere on the arm 3 may become established. This response pattern will, however, be somewhat general in nature.

Now suppose further that the stimulus (weakened somewhat) is applied in a succession of additional trials always to a definitely limited area on the arm 3, and that the mobility of this organ is sufficient to supply a large variety of movements of its several parts. Then we shall have the larger circumstance repeated here, but only on a minute scale. As the arm contracts, elongates, bends this way and that, turns over somewhat on its longitudinal axis, and performs other avoiding movements under the influence of the stimulus, certain motor impulses again, as distinct from all others, will necessarily change concurrently with changes in the sensory impulses which the movements resulting from them effect, and again we shall have a finer specialization of response to local stimulation; and so on, down to the limits of mobility. Evidently the greater the mobility of any of the limbs and of their local surfaces the finer the coördinations will be which are set up, or the greater the discriminability as to place of stimulation. If two-point stimuli are used, the greater the mobility the shorter will be the distance between any two points that can be distinguished when stimulated either simultaneously or successively. Since discriminative action between two points near together will involve nearly the same muscle systems, it is easy to understand that successive stimuli at the two points will be distinguished much more readily than simultaneous stimuli. If the two stimuli occur together the sensory-impulse changes from both points of stimulation will take place concurrently with most of the motor impulses of each response system, making the conditions for discrimination much poorer than when the two points are stimulated successively.

Now applying these considerations to discrimination on the human body, we readily see why discriminability of any part should correlate rather highly with mobility; why successive two-point stimulation should be more finely discriminated than simultaneous; and, because it is easier on

the whole to give twisting movements of our limbs and body than longitudinal movements, why discriminations of two points transversely arranged should be superior to that of points longitudinally arranged. It is usually held that the fineness of discrimination on any area depends on numerousness and proximity of nerve-endings supplied to that area. This is a very improbable explanation of the facts, one that probably puts the cart before the horse. Sensitivity to a single-point stimulus does not seem to be lacking on any area big enough to be separately stimulated with a moderately intense stimulus, however blunt discrimination may be at that point; moreover, stimulation at any point will usually result in tensions of the skin over considerable areas. So it appears that very small areas, even on the back, where discrimination is very poor, all have their supply of nerves, impulses from which, if mobility only permits, easily become associated with different efferent impulses to specific muscle systems. But if the member is large, like one's back, and not discriminatively mobile as to different parts, two-point discrimination, as well as localization of single points, must remain very poor, however numerous the sensory end organs. If there really is as much difference in the proximity of sensory nerve-endings on parts having very different discriminability as is usually assumed—but nowhere, so far as I am aware, proved—this difference can only, in the view here stated, be a *result* of the discriminability or of the mobility, not a *cause* of it. Stimulation even of moderate intensity on any part of the skin undoubtedly sets up impulses in a number of nerves varying in number and distribution with variations in the stimulus. Practice effects are thus comprehensible as increasingly effective utilizations of different groupings of sensory impulse factors whose variety is never, or seldom, quite exhausted. This fact, coupled with the additional one of increasing differentiation and specialization in the motor impulses with practice, leaves room for marked improvement in discriminability with training, especially when errors are carefully checked so as to make a difference to the subject in desirable results between effective and ineffective discrimination.

The illusion of displacement of a cutaneous stimulus toward the more movable parts of the bodily surface immediately adjoining the point stimulated is just what is to be expected on the basis of the view here outlined. Speaking generally we may say that wherever on the bodily surface mobility is greatest 'behavior units' are correspondingly large as compared with units measured on a physical scale; or, in subjective terms, the distance between any two discriminable points feels greater than it actually is physically.

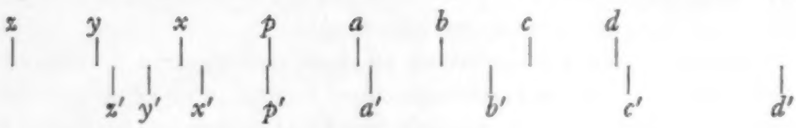

Fig. 1

Thus if *a*, *b*, *c*, and *d*, Fig. 1, are physically equidistant, measuring distally, on the arm from *p*, and if *x*, *y*, and *z* are likewise equidistant, measuring proximally, we may regard the corresponding behavior units as those marked off by *a′*, *b′*, *c′*, and *d′* in the direction of increasing mobility and by *x′*, *y′*, and *z′* in that of decreasing mobility. The equal physical units are thus represented respectively by progressively increasing behavior units in the distal direction and by progressively decreasing behavior units the other way from any point *p*. Hence if a blindfolded subject (S) is stimulated successively with pairs of simultaneous stimuli varying randomly in the distance of separation and if one point each time is placed at or near *p* while the other is located either distally or proximally from that point, S, if asked to estimate the distance of separation between each pair of points, will overestimate, on an average, the physical distances in the distal direction and underestimate those in the proximal direction. If instead of two points, only one is used each time to stimulate S's left arm while S is asked to touch with a wooden pencil held in his right hand the point that each time has just been stimulated, the average error will be in the distal direction. The theoretical point beyond which the behavior units increase is

this time further toward, or in, S's body, and the right hand, being gaged by the 'behavior units,' is placed too far out. Experimentally established results, so far as they bear on the matter, seem to support this hypothesis, but it is desired to give it further tests. The results predicted cannot be expected universally but only in general, because of the greater mobility at certain joints than at more distal points, a condition breaking the rule of general increased mobility toward the extremities. For most clear results, areas having the greatest variation in mobility should be stimulated, such, for instance, as those about the mouth.

Certain visual phenomena support our view in a striking manner. Let S look through the 'wrong' end of a pair of opera glasses (held in his left hand) at a dot on a sheet of clean paper lying before him in a horizontal position while with his right hand he strikes out quickly with a pencil to hit the dot. S declares that the dot appears to be much further away than it really is by the usual physical tests, but in spite of the knowledge of this fact and of his having seen the dot before using the glasses he at first strikes beyond it, probably two or three inches. Rapid improvement is made in subsequent trials, due to S's knowledge of each error as it is made, but the average of a number of trials gives an error of the same kind as the distal error in locating a stimulated point on the arm. If in this case employing vision, S closes his eyes each time before the pencil strikes the sheet, and if a clean sheet is used each time so that he does not become aware of the nature of his errors, the average error is of course considerable. It must not be overlooked that in the tactual experiment the knowledge or the general nature of the error also comes with each trial.

In our laboratory an experiment has been devised to illustrate the processes of acquiring motor control by association of impulses on the principle outlined on a foregoing page, but with the difference that the responses are directed to some other object than the subject's own cutaneous areas. The experimenter blindfolds the subject and instructs him as follows:

"I shall ask you to trace slowly with a lead pencil some simple figures. You must be blindfolded throughout the experiment, and your pencil must not be raised from the paper during the tracing of any one figure. At the beginning of each tracing I will put the point of your pencil on the place from which you are to start, and you must trace slowly and attend carefully. I will touch your left hand with my pencil whenever your pencil point crosses the right place, and will tap rapidly on your hand whenever you are tracing in the right direction and place. When you depart from the figure you are tracing the tapping will cease. If at any time you get hopelessly lost I shall put your pencil back on the right place, but this will occur very seldom; you must try to correct your own errors, and must learn by your own efforts. If you trace in the wrong direction on the line, or cross it too far from the place to which you have traced, you will receive no touch signals. Continue to trace the successive figures without asking questions, but let me know at the end of any tracing if you are tired, so you can take a short rest. Any time that you forget any essential part of these instructions I shall be pleased to read them through again for you."

Several simple forms for tracing have been used in this experiment—such as large S's, D's, 8's, 7's, and some consisting of two squares joined at a corner, each of these forms being about 2 inches in height. As a criterion of accuracy the lines of each form were marked off into quarter-inch sections so that the experimenter could force the subject to trace over each section, or very close to the side of it, in its proper order. As would be expected under the indefiniteness of S's problem, the first responses are to a high degree purely random, some subjects wandering rather freely over large areas of the sheet and occasionally having to be put back into position, while others make very small and cautious movements controlled more closely by the tapping signals. While we have yet no collection of reliable data on the tracing of any specific form, we find that on the whole progress is rapid, though frequently irregular, so that by the time S has traced about twelve identical forms a fairly accurate tracing habit ('set' for general outline of tracing and for guidance by the tapping signals) is built up. It is difficult in the first part of the learning to count errors accurately by any criterion,

but it is very obvious to anyone who examines the drawings that on the whole there is a rapid gain in accuracy. With nine subjects in one experiment the geometric average of decrease in time of each trial over the next preceding one in twelve successive tracings was 13 percent. Since individual differences and fluctuations in successive trials by any individual were considerable, this can give but a very general conception of the decrease in time with practice; but it is to be noted that even with such rapid decrease in time the decrease in errors was also considerable. That is, better and better work was done in the successive trials, but it was also done more quickly each trial than in the preceding trial. These results leave little doubt that the early practice afforded the infant in adjustment to local stimuli, even though the circumstances are very different from those in our experiment, yield very great results in a few months; so it is not surprising to find the young child's orientation to bodily stimuli so accurate as to lead many observers to the conclusion that its basis is mainly innate.

At the end of twelve trials each subject was asked to draw while still blindfolded, first with the right hand and then with the left, a figure as nearly as possible like the one he had been tracing; next he was asked to draw the figure again with the right hand by the aid of vision, but without having seen his previous drawings. Then, without being under any limitations of time he was asked to write out fully the answers to these questions: "About when and under what circumstances did you first realize that the figures you were drawing were all alike? At about what stage did you begin, and how, to get the form of the figure in mind, and with what was it related to give it permanency? Did you react to it mainly as a visual or as a muscular 'picture'"? Even though S is at first mainly taken up with the tapping signals the reports show that the form of the figure traced begins to stand out in about the third trial, and that this set for form becomes an additional control factor in the guidance of the movements, often, when it is wrong, becoming an obstacle to the learning. S's figures drawn after the experiment, both when blindfolded

and with the aid of vision, are frequently distorted and considerably too large or too small, but why should we expect a close agreement between the motor figure built up in one way and the visual figure established more consistently as to size and shape by numerous well-checked experiences?

The experiment illustrates how a deaf person, by careful checking and direction in vocal behavior based on a trainer's auditory sensitivity, may acquire accurate speaking habits, the degree of accuracy depending on the rigidity of the checking of wrong vocalizations, accents, etc. It also indicates how, under slightly different circumstances, habits of orientation to different local stimuli on the skin may be built up on the principle outlined in the foregoing pages. In the process of acquiring these orientation habits the most mobile parts of the body will not only get the most practice but they will also on the whole get the most accurate checking by differentiable results. There is of course no room to deny the fact that many rather specific orientation responses are based on innate mechanisms (*e.g.*, responses like sneezing, clasping, winking the eyes, and other reflexes); their presence will serve only to hasten and define the orientation habits set up by the trial-and-error process outlined in this paper. Thus there can be no conflict between nativists and empiricists except as to the degree of influence of each of these factors in any specific case, a matter that is open to observation and experimentation.

It may be possible to demonstrate empirically by a modification of our experiment the actual building up and specialization of orientation habits to bodily stimuli, and to investigate transfer effects and also effects of unilateral and other impediments or obstructions to free activity. Studies of infants' responses to local stimuli through long periods of time as well as studies of guinea pigs, rats, and of other animals by similar methods would give valuable data. Language responses would not need to be involved in the case of the infants, although they are convenient aids when older persons are used. The utter failure of the infant to respond specifically to certain painful stimuli is well known

but it has received little scientific study, even though there lies between this stage of beginning, non-specific responses, and that of accurate orientation in the child of eight or ten years to spatial and temporal stimulation of various kinds, a period of most interesting and instructive specialization of orientation habits. How this development comes about, how much of it is attributable to mere maturation of innate structures and how much to the sort of more or less accurate checking of random responses suggested in the foregoing paragraphs, to what extent it can be aided and enhanced by specifically devised circumstances, etc.—these are problems which must be experimentally determined, and which are closely related to other studies of learning processes. Studies of this nature are of supreme importance to the development of psychology, especially since in the past psychologists have been inclined to attribute most higher forms of learning to mysterious forces in 'ideas,' 'purposes,' 'meanings,' 'pleasures and pains,' etc. We have not in this paper had space for even mentioning the deeper problems of how the modifications in learning come about in their more puzzling details. The general outline of the processes of specialization in orientation responses to bodily stimulation here given is probably definite and objective enough, so far as it goes, to be acceptable to the several advocates of different theories of learning.

One step further must be taken here, that of distinguishing between *orientation tendency* and *habit.* In most cases these terms may be used interchangeably, especially if 'habit' is taken to mean not particular response but acquired neuromuscular organization for such response under proper conditions of stimulation. 'Tendency' is of course a broader term than 'habit,' involving also any innate organization for responses that actually exists at birth or that results from maturation at any later date. Presumably any orientation response will be made whenever the conditions for its excitation are adequate. When we say that a response is in readiness but is held back as 'meaning,' or 'tendency,' so that a particular situation or thing means a certain kind of response or 'anticipated experience,' we are beclouding the

issue, in my judgment, unless what we say is only that some factors in the situation are not yet adequate for the arousal of the act, that the conditions of stimulation (internal and external) are not yet complete for the act to take place. If I *mean* when I get to the library to read a certain chapter in a treatise on physiology, the only reason why I do not now read it is that conditions for this act are not fulfilled—that the act is impossible under the conditions. To 'appreciate as meaning without the necessity of responding in an overt manner' (Carr) is simply *to respond now to a present situation in a manner that is different from the response previously made to a similar, but not identical, situation.*

Several advantages result from conceiving local signature merely as orientation tendency instead of as some mysterious guidance of action by assumed sensory signs or as some sort of reference to location by these signs. (1) The various aspects of the problem become subject to experimental investigation of an objective nature. Young animals and infants may be used as subjects and investigations can be made to determine how readily and in what manner responses to various local stimuli become organized. (2) The explanations work just as well with stimuli and nerve impulses that are unconscious as with those that are conscious; the question of consciousness has no relevancy and need not enter at all either into the experimentation or the interpretation of results. (3) On the proposed view we have some very definite things between which assumed associations become established,—two classes of changes in nerve impulses, changes in the sensory nerves affected by the stimulation and in the proprioceptive fibers involved in the *relevant* movements, on the one hand, and changes in the motor nerves connected with the relevant effectors, on the other. No other theory of local signs has suggested anything so definite as this as a basis for the associations. (4) Differences between nativists and empiricists, which on subjective views have been irreconcilable, at once become resolvable, since any degree of specialization in response due to innate conditions, when established as a fact, serves only as a basis for further

organization by habit formation as necessities demand. Differences between representatives of these 'schools' can only be matters of degree of innateness, easily subject to investigation and statable in the form of learning curves or of amount of increased efficiency resulting from well-checked practice, indicating degree of training effects possible. (5) The interminable controversies as to whether sensations have extension inherent in themselves or whether space can be built up by association of non-extensible mind-stuff become useless and even ridiculous. (6) Local signature and perception, both spatial and temporal, become only special aspects of the general field of learning, and, like other forms of learning, subject to objective experimental study.

THE CONCEPT OF RETROACTIVE INHIBITION

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Now that a fair amount of experimental work has been reported under the heading of retroactive inhibition one may well raise the question of what the term should or should not include. A subject has learned something (call it *A*). A second learning follows and it is found that this 'after activity' (call it *B*) has in some way worked detrimentally upon the original learning *A*. This phenomenon has come to be called, in a rather indiscriminate way, retroactive inhibition, following the terminology used by Müller and Pilzecker. As one studies over the experimental results one wonders if it is not time to attempt a more rigid definition of these terms.

One question which occurs is whether the term should include both temporary blockings and permanent effacements of learning. After a given learning which is followed by an interpolated work activity, one may find a temporary blocking with an ultimate recall of the original learning. On the other hand, one may find a complete (at least from the standpoint of recall) obliteration of the associate.

A second question of much interest is whether our term should include the phenomenon of retrograde amnesia as reported by the psychiatrist. Regarded only from the standpoint of the end results the phenomenon appears similar or identical to retroactive inhibition. But are the conditions and processes in the two cases sufficiently alike to justify the merging of these phenomena under one concept?

A third question arises as to whether we ought to include any form of affective or emotional influence which operates retroactively. Or should we rigidly exclude all such retroactive influences? This question necessarily involves the preceding one.

Finally, can we bring all the experimental facts under one concept? Or is it necessary to split up our concept and limit the term retroactive inhibition more rigidly? We have in mind the major experimental works by De Camp,[1] Heine,[2] Müller and Pilzecker,[3] Robinson,[4] Tolman,[5] Webb[6] and Skaggs.[7] The writer is convinced that the present usage of the term is loose and too general in the light of the facts brought to light by these investigators. We need to define in terms of means or processes rather than in terms of end results.

In regard to the first question it is very difficult to decide at the present time. We have no way of testing any retroactive 'detrimental influence' excepting by testing efficiency in recall or recognition. Any decrease in efficiency may be due (1) to a weakening of the associative connections wrought by the 'after activity,' (2) to wrong associative tendencies established by the 'after activity,' or (3) to factors operative at the time of the attempted recall.

Let us assume that the subject has learned to some extent the following paired nonsense syllables:

zig................bix
waf................lac
rav................jur, etc.

After the learning he participates in the doing of some prescribed task or learning activity (*B*). When, in the testing,

[1] De Camp, J. E., 'A Study of Retroactive Inhibition,' Psychol. Monog., 1915, **19**.

[2] Heine, R., 'Über Widererkennen und ruckwirkende Hemmung,' *Zsch. f. Psychol.*, 1914, **68**, 161–236.

[3] Müller and Pilzecker, 'Experimental Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, 1900, *Erg.-Bd. I*, pp. 1–288.

[4] Robinson, E. S., 'Some Factors Determining the Degree of Retroactive Inhibition,' Psychol. Monog., 1920, **27**.

[5] Tolman, E. C., 'Retroactive Inhibition as Affected by Conditions of Learning,' Psychol. Monog., 1917–1918, **25**.

[6] Webb, L. W., 'Transfer of Training and Retroaction,' Psychol. Monog., 1917, **24**.

[7] Skaggs, E. B., 'Further Studies in Retroactive Inhibition,' Psychol. Monog., 1925, **34**.

the syllable *zig* is shown the subject reproduces the associate, *bix*, but only after an unusually long time. Here was a temporary blocking. The subject gives something like the following introspections: "*Zig* appeared. I felt that I ought to know the associate well but it didn't come. *Lac* came to my mind as a fairly clear visual image but there accompanied it the feeling that it was not the proper associate. At once the syllable *bix* came and I released the key." Here we find a long reaction time as compared to other experiments where there was no interpolated work. The associate ultimately came, the interference being only temporary. But there are several possible interpretations. One may say that there was a weakening of the associative connection between *zig* and *bix*. Or putting it another way we may say that the bond of connection between these two syllables never attained its maximum strength due to some disturbance of a 'setting process.' Or one might say that the reason for the long reaction time was due to wrong associative tendencies set up, due to the interrelations between original learning and work activity. Finally one may say that the interference was due to factors operative at the time of recall. What is the relative importance of each? The writer sees no possibility of answering this question and regards the experimental results obtained as equivocal in significance.

On the other hand the subject may be unable to get the associate *bix* when the syllable *zig* is shown. He reports that nothing came. Again we wonder why the associate has been lost. Was it due to some detrimental influence which operated backwards upon the original learning due to the interpolated work or was it due to some blocking process exerted at the time of recall? One might argue that we should rule out all temporary interference cases when looking for evidences of true retroactive inhibition, which would also mean that reaction times would not be considered. If the associate or original learning is utterly obliterated then one might argue for a true case of retroaction. We are inclined to favor ths last view, holding that temporary blockings with ultimate recall of the original learning is very liable to

indicate the presence of factors working *at the time of recall.* This would then be a case of what Pillsbury calls reproductive inhibition.[8] Whether some cases now listed as reproductive inhibition are really cases of retroactive inhibition is a question.

The second question which we have raised is equally difficult to settle and one cannot discuss it without involving the third. Most cases of so-called retrograde amnesia are characterized by two circumstances. First, there is an emotional shock, and second, there is concerned an unusually unstable nervous system. As far as end results are concerned the retrograde amnesia cases (at least some of them) appear identical to the cases classed by the psychologists under the heading of retroactive inhibition. The question is whether we want to make our concept so broad that it will include these emotional conditions and the nervous instability concerned. In the experimental work done under the heading of retroactive inhibition, neither of these conditions has obtained. The subjects have been normal and the emotional factors have not been involved. In the light of all this, the writer suggests that we regard retrograde amnesia as something different from the phenomenon signified by the usual use of the term retroactive inhibition.

Should retroaction include emotional and affective influences or be restricted to a coldly intellectual situation? As far as emotional influences are concerned, we suggest that wherever there is found a detrimental retroactive influence on a given learning which is decidedly emotional in nature we call it emotional retroaction and keep it distinct from retroactive inhibition. But the case is more difficult when we consider the affections. There is probably no learning that does not involve some affective tone. The experiments reported on retroactive inhibition have always involved learnings and interpolated work. It seems unreasonable to think that all such activities were purely neutral as far as affection was concerned. So, granting a good case of retroaction, one wonders just how much was due to the affective

[8] Pillsbury, W. B., 'Fundamentals of Psychology,' 1922, 385 *f.*

factors (however slight) and how much was due to the non-affective processes.

Certainly we must believe that affective processes are important in learning, retention and recall. Of course when we refer to pleasantness or unpleasantness as reinforcing or inhibiting processes, we mean the correlated neurological processes involved. A goodly amount of experimental work is now at hand dealing with the affective processes. As fairly representative we may mention the work by Smith [1] and Griffitts.[2] Smith finds an influence exerted by affection on recall; one affective state (not to be identified with either the traditional pleasantness or unpleasantness) facilitates, and another inhibits, recall. Griffitts finds little or no evidence of repression (inhibition?) but his work indicates that any kind of affective state 'is favorable to the recall of the stimulus.' In the light of these suggestions we fear that the past experimental work cannot tell us much about the part played by the affective dispositions. It seems better to consider retroactive inhibition as over and above any affective influences for the present.

Our fourth question has been partly answered by the discussion of the preceding problems. After we have restricted retroactive inhibition to those permanent effacements of a given learning wrought in a normal person by some succeeding mental activity, over and above emotion and affection, a further question of limitation remains. The writer feels that two fundamental sets of facts have now been sufficiently established to force a further limitation on our concept of retroaction. The temporal position of the interpolated work and the degree of the mental effort and concentration involved in the work activity following the learning appears very important. Müller and Pilzecker suggested that the 'after activity' interfered with some sort of 'perseveration' process. The associative bonds grew stronger with time,

[1] Smith, W. W., 'Experiments on Memory and Affective Tone,' *Brit. J. Psychol.*, 1920–21, 11, 236–250. (Smith seems to confuse 'emotion' and 'affection.' He deals with what is usually called 'affection,' not 'emotion.')

[2] Griffitts, C. H., 'Results of Some Experiments on Affection, Distribution of Associations, and Recall,' J. Exper. Psychol., 1920, 3, 447–464.

providing no mental activity was introduced soon afterwards. On the other hand, it now appears that the degree of similarity between the original learning and the interpolated work is important in this retroactive process. If original learning and interpolated learning are identical, then there is mere repetition and no retroaction. But as these two are made more and more different there comes a time of maximum retroaction.[9] This matter of similarity seems to be over and above the factors of temporal position and degree of mental activity.

The writer suggests that we restrict the term 'retroactive inhibition' to those cases where temporal position and degree of mental concentration are the essential factors. We are growing more and more to the conviction that the *modus operandi* of these factors on the one hand, and the 'degree of similarity factors' on the other, are sufficiently different to warrant a splitting of the original concept. We might reserve the term retroactive inhibition for that phenomenon whereby the original bonds of association are weakened, either, as Müller and Pilzecker suggested, due to interference with a 'perseveration' process, or to some positive weakening process, the nature of which we know nothing. When we find a retroactively detrimental influence present, and due essentially to the degree of similarity between original and interpolated (succeeding) learning or other mental activity, we might use some other term than retroactive inhibition. The neurological processes are probably different. In the latter case the original learning is disturbed only from the standpoint of recall. This is probably due to the setting up of wrong associative tendencies, which are likely to lead to confusion when one attempts to recall. In fact one wonders if this retroaction due to a certain degree of similarity between original and succeeding learning might not be included under the concept of associative inhibition, as formulated by Müller and Schumann, and mentioned in several texts.[10] We would not say that these phenomena are identical but we

[9] See monograph by writer, *op. cit.*, p. 32.

[10] *Cf.*, *e.g.*, Pillsbury, W. B., 'Fundamentals of Psychology,' 1922, 379 *f.*

strongly suspect that the disturbances wrought on a previous learning by a succeeding learning which contains partial identities operates as associative inhibition. Here we are not dealing with a pure case of weakening of the original associative bonds but rather with the starting of a series of dangerous and confusing extraneous association tendencies. This, along with disturbing factors operative at the time of recall, could lessen the efficiency of the reproduction although one would not expect a permanent loss of the learned associates.

If our way of looking at the matter is at all sound then it would not be unreasonable to give the following interpretation to the experimental results. If a detrimental retroaction is found when one is using, as a succeeding learning series, material or method of a degree of similarity which yields a maximum inhibition, then we are dealing with a case which should probably be classed as associative inhibition. However, the experimental results indicate that there is likely to be a detrimental influence wrought due to the intensity or temporal position of the mental activity itself. This would probably involve some true retroactive inhibition. On the other hand, one might well turn the tables and reply that we can (probably) never have two absolutely dissimilar learnings and consequently we could never test pure retroactive inhibition in the sense in which it is used above. Probably all one can do is to say that the detrimental influence found is due *essentially* to retroactive inhibition or *essentially* to associative or other forms of inhibition.

In conclusion then we offer for criticism a definition and concept of retroactive inhibition which is very restricted. It must deal with a permanent loss of an associate and must exclude all emotional and affective influences. It must exclude all cases of decreased efficiency in recall brought about by wrong associative tendencies due to partial identities. It must exclude all cases of lessened efficiency in recall due to factors operating at the time of recall and now generally known as reproductive inhibitions. In a word we must revert to the notion of Müller and Pilzecker, although we

need not literally accept their explanation in terms of the perseveration tendency. One mental activity must 'blot out' a preceding activity as a result of some positive (and certainly unknown at present) influence. The writer still feels that the perseveration notion advanced by Müller and Pilzecker is our best hypothesis.

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